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EXPERIMENTAL ENQUIRY

CONCERNING THE

Natural Powers of Wind and Water

TO TURN

MILLS AND OTHER MACHINES

DEPENDING ON A

CIRCULAR MOTION.

AND AN

EXPERIMENTAL EXAMINATION

OF THE

QUANTITY AND PROPORTION

OF MECHANIC POWER

Neceſſary to be employed in giving different degrees of VELOCITY
to HEAVY BODIES from a STATE of REST.

ALSO

NEW FUNDAMENTAL EXPERIMENTS

UPON THE

COLLISION OF BODIES.

WITH FIVE PLATES OF MACHINES.

BY THE LATE MR. JOHN SMEATON, F. R. S.

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ADVERTISEMENT.

THE ſubjects of the following ſheets, as branches of practical mechanics, are intereſting; and the manner in which they are treated render them important.

Previous to theſe experiments, the ſuperior power of overſhot water-wheels was not only doubted, but ſome authors of conſiderable celebrity had given a preference to underſhot wheels.—The experiments made, and here fully explained by Mr. SMEATON, *(who, as a practical mechanic, muſt ever be rated in the firſt claſs) ſhew the fallacy of former concluſions, and the great ſuperior power of overſhot wheels: the lapſe of time ſince the publication of theſe principles, has confirmed their accuracy, and has eſtabliſhed a durable reputation for their author.*

Theſe Eſſays, publiſhed originally in the Philoſophical Tranſactions (the parts of which are now become ſcarce, and of high price on their account) are now offered to the public at a moderate purchaſe; to render them acceſſable to every ingenious enquirer, muſt tend to ſpread the very uſeful information they contain. And if to ſome perſons the price may appear dear, in anſwer it muſt be obſerved, that

that though the work is of the greateſt importance to the practical mechanic, the number of ſuch purchaſers cannot be eſtimated as numerous. To a ſuperior mind nothing can be more grateful, than imparting to others the advantages of that ſuperiority, which bountiful Heaven has beſtowed on enlightened Genius.

AN

EXPERIMENTAL ENQUIRY

CONCERNING THE

NATURAL POWERS

OF

WATER AND WIND

To turn *Mills* and other *Machines*, depending on

A CIRCULAR MOTION;

Read before the Royal Society, May 3, & 10, 1759.

WHAT I have to communicate on this ſubject was originally deduced from experiments made on working models, which I look upon as the beſt means of obtaining the outlines in mechanical enquiries. But in this caſe it is very neceſſary to diſtinguiſh the circumſtances in which a model differs from a machine in large; otherwiſe a model is more apt to lead us from the truth than towards it. Hence the common obſervation, that a thing may do very well in a model that will not anſwer in large. And, indeed, though the utmoſt circumſpection be uſed in this way, the beſt ſtructure of machines cannot be fully aſcertained, but by making trials with them, when made of their proper ſize. It is for this reaſon that though the models referred to, and the greateſt part of the following experiments, were made in the years 1752 and 1753, yet I deferred offering them to the Society, until I had an opportunity of putting the deductions made therefrom in real practice, in a variety of caſes, and for various purpoſes; ſo as to be able to aſſure the Society that I have found them to anſwer.

PART I.

Concerning Underſhot Water-Wheels.

PLATE I. Fig. 1. is a perſpective view of the machine for experiments on water-wheels; wherein

A B C D is the lower ciſtern, or magazine, for receiving the water, after it has quitted the wheel; and for ſupplying

D E the upper ciſtern, or head; wherein the water being raiſed to any height required, by a pump, that height is ſhewn by

F G, a ſmall rod, divided into inches and parts; with a ſloat at the bottom, to move the rod up and down, as the ſurface of the water riſes and falls.

H I is a rod by which the ſluice is drawn, and ſtopt at any height required, by means of

K a pin, or peg, which fits ſeveral holes, placed in the manner of a diagonal ſcale, upon the face of the rod H I.

G L is the upper part of the rod of the pump, for drawing the water out of the lower ciſtern, in order to raiſe and keep up the ſurface thereof at its deſired height, in the head DE; thereby to ſupply the water expended by the aperture of the ſluice.

MM is the arch and handle for working the pump, which is limited in its ſtroke by

N, a piece for ſtopping the handle from raiſing the piſton too high; that alſo being prevented from going too low, by meeting the bottom of the barrel.

O is the cylinder, upon which a cord winds, and which, being conducted over the pullies P and Q, raiſes

R, the ſcale, into which the weights are put, for trying the power of the water.

S T, the two ſtandards, which ſupport the wheel, are made to ſlide up and down, in order to adjuſt the wheel, as near as poſſible, to the floor of the conduit.

W the beam which ſupports the ſcale and pullies; this is repreſented as but little higher than the machine, for the ſake of bringing the figure into a moderate compaſs, but in reality is placed 15 or 16 feet higher than the wheel.

PLATE II. Fig. 2. is a ſection of the ſame machine, wherein the ſame parts are marked with the ſame letters as in Fig. 1. Beſides which

XX is the pump barrel, being 3 inches diameter, and 11 inches long.

Y is the piſton; and

Z the fixed valve.

GV is a cylinder of wood, fixed upon the pump-rod, and reaches above the ſurface of the water: this piece of wood being of ſuch a thickneſs, that its ſection is half the area of that of the pump-barrel, will cauſe the ſurface of water to riſe in the head, as much while the piſton is deſcending, as while it is riſing: and will thereby keep the gauge-rod F G more equally to its height.—*Note*, The arch and handle M M is here repreſented on a different ſide to what it is ſhewn in the preceding figures, in order that its dimenſions may the better appear.

a a ſhews one of the two wires which ſerve as directors to the float, in order that the gauge rod F G may be kept perpendicular; for the ſame purpoſe alſo ſerves *w*, a piece of wood with a hole to receive the gauge-rod, and keep it upright.

b is the aperture of the ſluice.

c c a kant-board, for throwing the water more directly down the opening *c d*, into the lower ciſtern; and

c e is a ſloping board, for bringing back the water that is thrown up by the floats of the wheel.

Fig. 3. repreſents one end of the main axis, with a ſection of the moveable cylinder, marked O in the preceding figures.

A B C D is the end of the axis; whereof the parts

B and D are covered with ferrules or hoops of braſs.

E is a cylinder of metal; whereof the part marked

F is the pivot or gudgeon.

c c is

c c is the ſection of an hollow cylinder of wood, the diameter of the interior part being ſomewhat larger than the cylindrical ferrule B.

a a is the ſection of a ferrule of braſs, driven into the end of the hollow cylinder, and which is adjuſted to that marked B, ſo as to ſlide freely thereupon, but with as little ſhake as poſſible.

b b, *d d*, *g g*, repreſent the ſection of a braſs ferrule, plate, and ſocket, fixed upon the other end of the hollow cylinder; the ſocket *d d* being adjuſted to ſlide freely upon the cylinder E, in the ſame manner as the ferrule *a a* ſlides upon the cylinder B: the outer end of the ſocket at

g g is formed into a ſort of button; by puſhing whereof, the hollow cylinder will move backwards and forwards, or turn round at pleaſure upon the cylindrical parts of the axis B and E.

e e, *i i*, *o o*, repreſent the ſection of a braſs ferrule, alſo fixed upon the hollow cylinder: the edge of this ferrule

e e is cut into teeth, in the manner of a *contrate* wheel; and the edge thereof

o o is cut in the manner of a ratchet.

Of conſequence, when the plate *b d d b* is puſhed cloſe to the ferrule D, the teeth of the ferrule *e e* will lay hold of

G, a pin fixed into the axis; by which means the hollow cylinder is made to turn along with the wheel and axis: but being drawn back by the button *g g*, the hollow cylinder is thereby diſengaged from the pin G, and ceaſes turning. *Note,* The weight in the ſcale is prevented from running back, by a catch that plays in and lays hold of the ratchet *o o*.

By this means the hollow cylinder upon which the cord winds and raiſes the weight, is put in action and diſcharged therefrom inſtantaneouſly, while the wheel is in motion: for, without ſome contrivance of this kind, it would not be eaſy to make this ſort of experiments with any tolerable degree of exactneſs.

The uſe of the apparatus now deſcribed will be rendered more intelligible, by giving a general idea of what I had in view; but as I ſhall be obliged to make uſe of a term which has heretofore been the cauſe of diſputation, I think it neceſſary to aſſign the ſenſe in which I would be underſtood to uſe it; and in which I apprehend it is uſed by practical *Mechanicks*.

The word *Power*, as uſed in practical mechanicks, I apprehend to ſignify the exertion of ſtrength, gravitation, impulſe, or preſſure, ſo as to produce motion: and by means of ſtrength, gravitation, impulſe, or preſſure, compounded with motion, to be capable of producing an effect: and that no effect is properly mechanical, but what requires ſuch a kind of power to produce it.

The raiſing of a weight, relative to the height to which it can be raiſed in a given time, is the moſt proper meaſure of power; or, in other words, if the weight raiſed is multiplied by the height to which it can be raiſed in a given time, the product is the meaſure of the power raiſing it; and conſequently, all thoſe powers are equal, whoſe products, made by ſuch multiplication, are equal: for if a power can raiſe twice the weight to the ſame height; or the ſame weight to twice the height, in the ſame time that another power can, the firſt power is double the ſecond: and if a power can raiſe half the weight to double the height; or double the weight to half the height, in the ſame time that another can, thoſe two powers are equal. But *note*, all this is to be underſtood in caſe of ſlow or equable motion of the body raiſed; for in quick, accelerated, or retarded motions, the *vis inertiæ* of the matter moved will make a variation.

In comparing the effects produced by water-wheels with the powers producing them; or, in other words, to know what part of the original power is neceſſarily loſt in the application, we muſt previouſly know how much of the power is ſpent in overcoming the friction of the machinery, and the reſiſtance of the air; alſo what is the real velocity of the water at the inſtant that it ſtrikes the wheel; and the real quantity of water expended in a given time.

From the velocity of the water, at the inſtant that it ſtrikes the wheel, given, the height of head productive of ſuch velocity can be deduced, from acknowledged and experimented principles of hydroſtatics: ſo that by multiplying the quantity, or weight of water, really expended in a given time, by the height of a head ſo obtained; which muſt be conſidered as the height from which that weight of water had deſcended in that given time; we ſhall have a product, equal to the original power of the water, and clear of all uncertainty that would ariſe from the friction of the water, in paſſing ſmall apertures; and from all doubts, ariſing from the different meaſure of ſpouting waters, aſſigned by different authors. On the other hand, the ſum of the weights raiſed by the action of this water, and of the weight required to overcome the friction and reſiſtance of the machine, multiplied by the height to which the weight can be raiſed in the time given, the product will be equal to the effect of that power; and the proportion of the two products will be the proportion of the *power* to the *effect*: ſo tha loading the wheel with different weights ſucceſſively, we be able to determine at what particular load, and velocity of the wheel, the effect is a *maximum*.

The manner of finding the real velocity of the water, at the inſtant of its ſtriking the wheel; the manner of finding the value of the friction, reſiſtance, &c. in any given caſe; and the manner of finding the real expence of water, ſo far as concerns the following experiments, without having recourſe to theory; being matters upon which the following determinations depend, it will be neceſſary to explain them.

To determine the Velocity of the Water ſtriking the Wheel.

It has already been mentioned, in the references to the figures, that weights are raiſed by a cord winding round a cylindrical part of the axis. Firſt, then, let the wheel be put in motion by the water, but without any weights in the ſcale; and let the number of turns in a minute be 60: now it is evident, that was the wheel free from friction and reſiſtance, that 60 times the circumference of the wheel would be the ſpace through which the water would have moved in a minute; with that velocity wherewith it ſtruck the wheel: but the wheel being incumbered by friction and reſiſtance, and yet moving 60 turns in a minute, it is plain that the velocity of the water muſt have been greater than 60 circumferences before it met with the wheel. Let now the cord be wound round the cylinder, but contrary to the uſual way, and put a weight in the ſcale; the weight ſo diſpoſed (which may be called the *counter-weight)* will endeavour to aſſiſt the wheel in turning the ſame way, as it would have been turned by the water: put therefore as much weight into the ſcale as, without any water, will cauſe it to turn ſomewhat faſter than at the rate of 60 turns in a minute; ſuppoſe 63; let it now be tried again by the water, aſſiſted by the weight; the wheel therefore will now make more than 60 turns; ſuppoſe 64: hence we conclude the water ſtill exerts ſome power in giving motion to the wheel. Let the weight be again increaſed, ſo as to make 64½ turns in a minute without water: let it once more be tried with water as before; and ſuppoſe it now to make the ſame number of turns with water as without, *viz.* 64½: hence it is evident, that in this caſe the wheel makes the ſame number of turns in a minute, as it would do if the wheel had no friction or reſiſtance at all; becauſe the weight is equivalent thereto; for was it too little the water would accelerate the wheel beyond the weight; and if too great, retard it; ſo that the water now becomes a *regulator* of the wheel's motion; and the velocity of its circumference becomes a meaſure of the velocity of the water.

In like manner, in ſeeking the greateſt product, or *maximum*, of effect; having found by trials what weight gives the greateſt product, by ſimply multiplying the weight in the ſcale by the number of turns of the wheel, find what weight in the ſcale, when the cord is on the contrary ſide of the cylinder, will cauſe the wheel to make the ſame number of turns the ſame way, without water; it is evident that this weight will be nearly equal to all friction and reſiſtance taken together; and conſequently, that the weight *in* the ſcale, with twice * the weight *of* the ſcale, added to the back or counter-weight, will be equal to the weight that could have been raiſed, ſuppoſing the machine had been without friction or reſiſtance; and which multiplied by the height to which it was raiſed, the product will be the greateſt effect of that power.

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* The weight of the ſcale makes part of the weight both ways.

The

The Quantity of Water expended is found thus:

The pump made uſe of for repleniſhing the head with water was ſo carefully made, that, no water eſcaping back by the leathers, it delivered the ſame quantity of water at every ſtroke, whether worked quick or ſlow; and as the length of the ſtroke was limited, conſequently the value of one ſtroke (or, on account of more exactneſs, 12 ſtrokes) was known, by the height to which the water was thereby raiſed in the head; which, being of a regular figure, was eaſily meaſured. The ſluice, by which the water was drawn upon the wheel, was made to ſtop at certain heights by a peg; ſo that when the peg was in the ſame hole, the aperture for the effluent water was the ſame. Hence the quantity of water expended by any given head, and opening of the ſluice, may be obtained: for, by obſerving how many ſtrokes a minute was ſufficient to keep up the ſurface of the water at the given height, and multiplying the number of ſtrokes by the value of each, the water expended by any given aperture and head in a given time will be given.

Theſe things will be further illuſtrated by going over the *calculus* of one ſet of experiments.

Specimen of a Set of Experiments.

The ſluice drawn to the firſt hole.

The water above the floor of the ſluice	30	Inches.
Strokes of the pump in a minute —	39½	
The head raiſed by 12 ſtrokes —	21	Inches.
The wheel raiſed the empty ſcale, and made turns in a minute	80	
With a counter-weight of 1 lb. 8 oz. it made - -	85	
Ditto tried with water - - - -	86	

No.

No.	Weight. lb.	oz.	Turns in a Min.	Product.	
1	4	0	45	180	
2	5	0	42	210	
3	6	0	36¼	217½	
4	7	0	33¾	236¼	
5	8	0	30	240	maximum.
6	9	0	26½	238½	
7	10	0	22	220	
8	11	0	16½	181½	
9	12	* ceaſed working.			

Counter-weight, for 30 turns without water, 2 oz. in the ſcale.

N. B. The area of the head was 105,8 ſquare inches.
Weight of the empty ſcale and pulley, 10 oz.
Circumference of the cylinder, 9 inches.
Circumference of the water-wheel, 75 ditto.

Reduction of the above Set of Experiments.

The circumference of the wheel, 75 inches, multiplied by 86 turns, gives 6450 inches for the velocity of the water in a minute; $\frac{1}{60}$ of which will be the velocity in a ſecond, equal to 107,5 inches, or 8,96 feet, which is due to a head of 15 inches †; and this we call the *virtual* or *effective* head.

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* *N. B.* When the wheel moves ſo ſlow as not to rid the water ſo faſt as ſupplied by the ſluice, the accumulated water falls back upon the aperture, and the wheel immediately ceaſes moving.

† This is determined upon the common maxim of hydroſtatics, that the velocity of ſpouting waters is equal to the velocity that an heavy

The area of the head being 105,8 inches, this multiplied by the weight of water of the inch cubic, equal to the decimal ,579 of the ounce avoirdupoiſe, gives 61,26 ounces for the weight of as much water, as is contained in the head, upon 1 inch in depth, $\frac{1}{16}$ of which is 3,83 pounds; this multiplied by the depth 21 inches, gives 80,43 lb. for the value of 12 ſtrokes; and by proportion, 39½ (the number made in a minute) will give 264,7 lb. the weight of water expended in a minute.

Now as 264,7 lb. of water may be conſidered as having deſcended through a ſpace of 15 inches in a minute, the product of theſe two numbers 3970 will expreſs the *power* of the water to produce mechanical effects; which were as follows:

The velocity of the wheel at the *maximum*, as appears above, was 30 turns a minute; which multiplied by 9 inches, the circumference of the cylinder, makes 270 inches; but as the ſcale was hung by a pulley and double line, the weight was only raiſed half of this, *viz.* 135 inches.

The weight in the ſcale at the maximum	8 *lb.*	0 *oz.*
Weight of the ſcale and pulley -	0	10
Counter-weight, ſcale, and pulley -	0	12
Sum of the reſiſtance - -	9	6
	or *lb.* 9,375.	

Now as 9,375 lb. is raiſed 135 inches, theſe two numbers being multiplied together, the product is 1266, which expreſſes the effect produced at a maximum: ſo that the proportion of the *power* to the *effect* is as 3970 : 1266, or as 10 : 3,18.

heavy body would acquire in falling from the height of the reſervoir; and is proved by the riſing of jets to the height of their reſervoirs nearly.

But though this is the greateſt *ſingle* effect producible from the power mentioned, by the impulſe of the water upon an underſhot wheel; yet, as the whole power of the water is not exhauſted thereby, this will not be the true ratio between the *power* of the water, and the *ſum* of all the *effects* producible therefrom: for as the water muſt neceſſarily leave the wheel with a velocity equal to the wheel's circumference, it is plain that ſome part of the power of the water muſt remain after quitting the wheel.

The velocity of the wheel at the maximum is 30 turns a minute; and conſequently its circumference moves at the rate of 3,123 feet a ſecond, which anſwers to a head 1,82 inches; this being multiplied by the expence of water in a minute, *viz.* 264,7 lb. produces 481 for the power *remaining* in the water after it has paſſed the wheel: this being therefore deducted from the original power 3970, leaves 3489, which is that *part* of the power which is ſpent in producing the effect 1266; and conſequently the part of the power ſpent in producing the effect, is to the greateſt effect producible thereby as 3489 : 1266 : : 10 : 3,62, or as 11 to 4.

The *velocity of the water* ſtriking the wheel has been determined to be equal to 86 circumferences of the wheel per minute, and the *velocity of the wheel* at the *maximum* to be 30; the velocity of the water will therefore be to that of the wheel as 86 to 30; or as 10 to 3,5, or as 20 to 7.

The *load at the maximum* has been ſhown to be equal to 9 lb. 6 oz. and that the wheel ceaſed moving with 12 lb. in the ſcale: to which if the weight of the ſcale is added, *viz.* 10 ounces *, the proportion will be nearly as 3 to 4 between the load at the *maximum* and *that* by which the wheel is ſtopped.

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* The reſiſtance of the air in this caſe ceaſes, and the friction is not added, as 12 lb. in the ſcale was ſufficient to ſtop the wheel after it had been in full motion; and therefore ſomewhat more than a counterbalance to the impulſe of the water.

It is ſomewhat remarkable, that though the velocity of the wheel in relation to the water turns out greater than $\frac{1}{3}$ of the velocity of the water, yet the impulſe of the water in the caſe of a *maximum* is more than double of what is aſſigned by theory; that is, inſtead of $\frac{4}{9}$ of the column, it is nearly equal to the whole column.

It muſt be remembered, therefore, that in the preſent caſe, the wheel was not placed in an open river, where the natural current, after it has communicated its impulſe to the float, has room on all ſides to eſcape, as the theory ſuppoſes; but in a conduit, or race, to which the float being adapted, the water cannot otherwiſe eſcape than by moving along with the wheel. It is obſervable, that a wheel working in this manner, as ſoon as the water meets the float, receiving a ſudden check, it riſes up againſt the float, like a wave againſt a fixed object; inſomuch that when the ſheet of water is not a quarter of an inch thick before it meets the float, yet this ſheet will act upon the whole ſurface of a float, whoſe height is 3 inches; and conſequently was the float no higher than the thickneſs of the ſheet of water, as the theory alſo ſuppoſes, a great part of the force would have been loſt, by the water daſhing over the float*.

In

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* Since the above was wrritten, I find that Profeſſor Euler, in the Berlin Acts for the year 1748, in a memoir entitled *Maxims pour aranger le plus avantageuſment les machines deſtinees a elever de l'eau par le moyen de pompes*, page 192. § 9. has the following paſſage; which ſeems to be the more remarkable, as I do not find he has given any demonſtration of the principle therein contained, either from theory or experiment; or has made any uſe thereof in his calculations on this ſubject:——"Cependant dans ce cas puiſque "l'eau eſt reflechie, & qu'elle decoule ſur les aubes vers les cotés, "elle y exerce encore une force particuliere, dont l'effet de l'im- "pulſion

TABLE I.

Nº	Height of water in the ciſtern.	Turns of the wheel unloaded.	Virtual head deduced therefrom.	Turns at the maximum.	Load at the equilibrium.		Load at the maximum.		Water expended in a minute.	Power.	Effect.	Ratio of the power and effect.	Ratio of the velocity of the water and wheel.	Ratio of the load at the equilibrium to the load at the maximum.	Experiments.
	In.		In.		lb.	oz.	lb.	oz.							
1	33	88	15,85	30,	13	10	10	9	275,	4358	1411	10:3,24	10:3,4	10:7,75	At the 1ſt hole.
2	30	86	15,0	30,	12	10	9	6	264,7	3970	1266	10:3,2	10:3,5	10:7,4	
3	27	82	13,7	28,	11	2	8	6	243,	3329	1044	10:3,15	10:3,4	10:7,5	
4	24	78	12,3	27,7	9	10	7	5	235,	2890	901,4	10:3,12	10:3,55	10:7,53	
5	21	75	11,4	25,9	8	10	6	5	214,	2439	735,7	10:3,02	10:3,45	10:7,32	
6	18	70	9,95	23,5	6	10	5	5	199,	1970	561,8	10:2,85	10:3,36	10:8,02	
7	15	65	8,54	23,4	5	2	4	4	178,5	1524	442,5	10:2,9	10:3,6	10:8,3	
8	[illegible]	60	7,29	22,	3	10	3	5	161,	1173	328	10:2,8	10:3,77	10:9,1	
9	9	52	5,47	19,	2	12	2	8	134,	733	213,7	10:2,9	10:3,65	10:9,1	
10	6	42	3,55	16,	1	12	1	10	114,	404,7	117	10:2,82	10:3,8	10:9,3	
11	24	84	14,2	30,75	13	10	10	14	342,	4890	1505	10:3,075	10:3,66	10:7,9	At the 2d.
12	21	81	13,5	29,	11	10	9	6	297,	4009	1223	10:3,01	10:3,62	10:8,05	
13	18	72	10,5	26,	9	10	8	7	285,	2993	975	10:3,25	10:3,6	10:8,75	
14	15	69	9,6	25,	7	10	6	14	277,	2659	774	10:2,92	10:3,62	10:9,	
15	12	63	8,0	25,	5	10	4	14	234,	1872	549	10:2,94	10:3,97	10:8,7	
16	9	56	6,37	23,	4	0	3	13	201,	1280	390	10:3,05	10:4,1	10:9,5	
17	6	46	4,25	21,	2	8	2	4	167,5	712	212	10:2,98	10:4,55	10:9,	
18	15	72	10,5	29,	11	10	9	6	357,	3748	1210	10:3,23	10:4,02	10:8,05	The 3d.
19	12	66	8,75	26,75	8	10	7	6	330,	2887	878	10:3,05	10:4,05	10:8,1	
20	9	58	6,8	24,5	5	8	5	0	255,	1734	541	10:3,01	10:4,22	10:9,1	
21	6	48	4,7	23,5	3	2	3	0	228,	1064	317	10:2,99	10:4,9	10:9,6	
22	12	68	9,3	27,	9	2	8	6	359,	3338	1006	10:3,02	10:3,97	10:9,17	4th.
23	9	58	6,8	26,25	6	2	5	13	332,	2257	686	10:3,04	10:4,52	10:9,5	
24	6	48	4,7	24,5	3	12	3	8	262,	1231	385	10:3,13	10:5,1	10:9,35	
25	9	60	7,29	7,3	6	12	6	6	355,	2588	783	10:3,03	10:4,55	10:9,45	5th.
26	6	50	5,03	4,6	4	6	4	1	307,	1544	450	10:2,92	10:4,9	10:9,3	
27	6	50	5,03	26,	4	15	4	9	360,	1811	534	10:2,95	10:5,2	10:9,25	6th.
1.	2.	3.	4.	5.	6.		7.		8.	9.	10	11.	12.	13	

To face page 15.

In further confirmation of what is already delivered, I have adjoined a table, (TABLE I.) containing the reſult of 27 ſets of experiments, made and reduced in the manner above ſpecified. What remains of the theory of underſhot wheels, will naturally follow from a compariſon of the different experiments together.

Maxims and Obſervations deduced from the foregoing Table of Experiments.

Maxim I. *That the virtual or effective head being the ſame, the effect will be nearly as the quantity of water expended.*

This will appear by comparing the contents of the columns 4, 8, and 10, in the foregoing ſets of experiments; as for

Example 1ſt, taken from N°. 8. *and* 25 viz.

N°.	Virtual Head.	Water expended.	Effect.
8	7,29	161	328
25	7,29	355	785

Now the heads being equal, if the effects are proportioned to the water expended, we ſhall have by maxim 1ſt, 161 : 355 : : 328 : 723; but 723 falls ſhort of 785, as it turns out in experiment, according to N°. 25, by 62; the effect therefore of

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“ pulſion ſera augmenté; & perienex ce jointe a la theorie a fait
“ voir que dans ce cas, la força eſt preſque double: de ſorte qui'il
“ faut prendre le double de le ſection du fil d'eau pour ce qui repond
“ dans ce cas a le ſurface des aubes, pourvu qu'elles ſoient aſſez
“ larges pour recevoir ce ſupplement de force. Car ſi les aubes
“ nétoient plus larges que le fil, on trait d'eau on ne devroit prendre
“ que ne ſimple ſection, tout comme dans le premier cas, on l'aube
“ toute entire eſt pappee par l'eau.”

N°. 25, compared with N°. 8, is greater than according to the preſent maxim in the ratio of 14 to 13.

The foregoing example, with four ſimilar ones, are ſeen at one view in the following Table.

Examples.	N°. Tab. I.	Virtual Head.	Expence of Water.	Effect.	Compariſon.	Variation.	Proportional Variation.
		Inch.	*b*				
1ſt	8 25	7,29 7,29	161 355	328 785	161 : 355 : : 328 : 723	62+	14 : 13
2d	13 18	10,5 10,5	285 357	975 1210	285 : 357 : : 975 : 1221	11—	121 : 122
3d	22 23	6,8 6,8	255 332	541 686	255 : 332 : : 541 : 704	18—	38 : 39
4th	21 24	4,7 4,7	228 262	317 385	228 : 262 : : 317 : 364	21+	18 : 17
5th	26 27	5,03 5,03	307 360	450 534	307 : 360 : : 450 : 531	3+	178 : 177

Hence therefore, in comparing different experiments, as ſome fall ſhort, and others exceed the maximum, and all agree therewith, as near as can be expected, in an affair where ſo many different circumſtances are concerned, we may, according to the laws of reaſoning by induction, conclude the maxim true; *viz.* that the effects are nearly as the quanity of water expended.

Maxim II. *That the expence of water being the ſame, the effect will be nearly as the height of the virtual or effective head.*

This alſo will appear by comparing the contents of columns 4, 8, and 10, in any of the ſets of experiments.

Example 1ſt, of N°. 2. *and* N°. 24 *viz.*

N°.	Virt. Head.	Expence.	Effect.
2	15	264,7	1266
24	4,7	262	385

Now as the expences are not quite equal, we muſt proportion one of the effects accordingly: thus

by maxim 1ſt, 262 : 264,7 : : 385 : 389
and by max. 2d, 15 : 4,7 : : 1266 : 397

Difference - - - - 8

The effect therefore of N°. 24, compared with N°. 2, is leſs than according to the preſent maxim in the ratio of 49 : 50.

The foregoing, and two other ſimilar examples, are compriſed in the following Table:

Examples.	N°. Tab. I.	Virtual Head.	Expence of Water.	Effect.	Compariſon.	Variation.	Proportional Variation.
1ſt	2 24	15 4,7	264,7 262	1266 385	Max. 1ſt, 262 : 264,7 : : 385 : 319 Max. 2d, 15 : 4,7 : : 1266 : 397	8 —	49 : 50
2d	1 10	15,85 3,55	275 114	1411 117	Max. 1ſt, 114 : 275 : : 117 : 282 Max. 2d, 15,85 : 3,55 : : 1411 : 316	34—	8 : 9
3d	11 17	14,2 4,25	342 167,5	1505 212	Max. 1ſt, 167,5 : 342 : : 212 : 433 Max. 2d, 14,2 : 4,25 : : 1505 : 450	17—	25 : 26

Maxim III. *That the quantity of water expended being the ſame, the effect is nearly as the ſquare of its velocity.*

This will appear by comparing the contents of columns 3, 8, and 10, in any of the ſets of experiments; as for

Example 1ſt, of N°. 2. *with* N°. 24. *viz.*

N°.	Turns in a min.	Expence.	Effect.
2	86	264,7	1266
24	48	262	385

The velocity being as the number of turns, we ſhall have,

by max. 1ſt, 262 : 264,7 : 385 : 389

$$\text{and by max. 3d.}\left\{\begin{matrix}86^2 : 48^2\\ 7396 : 2304\end{matrix}\right\} : 1266 : 394$$

Difference - - - 5

The effect therefore of N°.24, compared with N°. 2, is leſs than by the preſent maxim in the ratio of 78 : 79.

The foregoing, and three other ſimilar examples, are compriſed in the following Table:

Examples.	N°. Tab. I.	Turns in a minute.	Expence of Water.	Effect.	Comparison.	Variation.	Proportional Variation.
1ſt	2 24	86 48	264,7 262	1266 385	Max. 1ſt, 262 : 264,7 :: 385 : 389 Max. 3d, $86^2 : 48^2$ 7396 : 2304 :: 1266 : 394	5 —	78 : 79
2d	1 10	88 42	275 114	1411 117	Max. 1ſt, 114 : 275 :: 117 : 282 Max. 3d, $88^2 : 42^2$ 7744 : 1764 :: 1411 : 321	39 —	7 : 8
3d	11 17	84 46	342 167,5	1505 212	Max. 1ſt, 167,5 : 342 :: 212 : 433 Max. 3d, $84^2 : 46^2$ 7056 : 2116 :: 1505 : 451	18 —	24 : 25
4th	18 21	72 48	357 228	1210 317	Max. 1ſt, 228 : 357 :: 317 : 496 Max. 3d, $72^2 : 48^2$ 5184 : 2301 :: 1210 : 538	42 —	12 : 13

Maxim IV. *The aperture being the ſame, the effect will be nearly as the cube of the velocity of the water.*

This alſo will appear by comparing the contents of columns 3, 8, and 10; as for

Example 1ſt, of N°. 1. *and* N° 10, *viz.*

N°.	Turns.	Expence.	Effect.
1	88	275	1411
10	42	114	117

Lemma. It muſt here be obſerved, that if water paſſes out of an aperture, in the ſame ſection, but with different velocities, the expence will be proportional to the velocity; and therefore converſely, if the expence is not proportional to the velocity, the ſection of the water is not the ſame.

Now comparing the water diſcharged with the turns of N°. 1, and 10, we ſhall have 88 : 42 : : 275 : 131,2; but the water diſcharged by N°. 10. is only 114lb. therefore, though the ſluice was drawn to the ſame height in N°. 10, as in N°. 1, yet the ſection of the water paſſing out, was leſs in N°. 10, than N°. 1, in the proportion of 114 to 131,2; conſequently had the effective aperture or ſection of the water been the ſame in N°. 10, as in N°. 1, ſo that 131,2 lb. of water had been diſcharged inſtead of 114, the effect would have been increaſed in the ſame proportion; that is

by the *Lemma*, 88 : 42 : : 275 : 131,2
by maxim 1ſt, 114 : 131,2 : : 117 : 134,5
and by max. 4th, $\left\{\begin{matrix} 83^3 : 42^3 \\ 681472 : 74088 \end{matrix}\right\}$: : 1411 : 153,5

Difference - - - 19

The

The effect therefore of 10, compared with N°. 1, is leſs than it ought to be by the preſent maxim in the ratio of 7 : 8.

The foregoing, and three other ſimilar examples, are contained in the following Table.

Examples.	N°. Tab. I.	Turns in a minute.	Expence of water.	Effect.	Compariſon.	Variation.	Proportional Variation.
1ſt	1 10	88 42	275 114	1411 117	Lemma. 88 : 42 :: 275 : 131,2 Max. 1. 114 : 131,2 :: 117 : 134,5 Max. 4. 88^3 : 42^3 :: 1411 : 153,5	9—	7 : 8
2d	11 17	84 46	342 167,5	1505 212	Lemma. 84 : 46 :: 342 : 187,3 Max. 1. 167,5 : 187,3 :: 212 : 237 Max. 4. 84^3 : 46^3 :: 1505 : 247	10—	23 : 24
3d	18 21	72 48	357 228	1210 317	Lemma. 72 : 48 :: 357 : 238 Max. 1. 228 : 238 :: 317 : 331 Max. 4. 72 : 48^3 :: 1210 : 355	4—	14 : 15
4th	22 24	68 48	359 262	1006 385	Lemma. 68 : 48 :: 359 : 253,4 Max. 1. 262 : 253,4 :: 385 : 372 Max. 4. 68^3 : 48^3 :: 1006 : 354	18+	20 : 19

Obſervations.

Obſervations.

Obſerv. 1ſt. On comparing column 2d and 4th, Tab. I. it is evident that the *virtual head* bears no certain proportion to the *head of water*; but that when the aperture is greater, or the velocity of the water iſſuing therefrom leſs, they approach nearer to a coincidence; and conſequently in the large openings of mills and ſluices, where great quantities of water are diſcharged from moderate heads, the head of water, and virtual head determined from the velocity, will nearly agree, as experience confirms.

Obſerv. 2d. Upon comparing the ſeveral proportions between the *power* and *effect* in column 11th, the moſt general is that of 10 to 3; the extremes 10 to 3,2 and 10 to 2,8; but as it is obſervable, that where the quantity of water, or the velocity thereof; that is, where the power is greateſt, the 2d term of the ratio is greateſt alſo: we may therefore well allow the proportion ſubſiſting in large works, as 3 to 1.

Obſerv. 3d. The proportions of *velocities* between the *water* and *wheel* in column 12, are contained in the limits of 3 to 1 and 2 to 1; but as the greater velocities approach the limit of 3 to 1, and the greater quantity of water approach to that of 2 to 1, the beſt general proportion will be that of 5 to 2.

Obſerv. 4th. On comparing the numbers in column 13, it appears, that there is no certain ratio between the *load* that the wheel will carry at its *maximum*, and what will totally ſtop it; but that they are contained within the limits of 20 to 19, and of 20 to 15; but as the effect approaches neareſt to the ratio of 20 to 15, or of 4 to 3, when the power is greateſt, whether by increaſe of velocity, or quantity of water, this ſeems to be the moſt applicable to large works; but as the load that a wheel ought to have, in order to work to the beſt advantage, can be aſſigned, by knowing the effect it ought to produce, and the velocity it ought to have in producing it; the exact knowledge of the greateſt load it will bear, is of the leſs conſequence in practice.

It is to be noted, that in all the examples under the three laſt of the four preceding maxims, the effect of the leſſer power falls ſhort of its due proportion to the greater, when compared by its maxim; except the laſt example of maxim 4th: and hence, if the experiments are taken ſtrictly, we muſt infer, that the effects increaſe and diminſh in an higher ratio than thoſe maxims ſuppoſe: but as the deviation is not very conſiderable, the greateſt being about 1-8th of the quantity in queſtion; and as it is not eaſy to make experiments of ſo compounded a nature with abſolute preciſion; we may rather ſuppoſe, that the leſſer power is attended with ſome friction, or works under ſome diſadvantage, which has not been duly accounted for; and therefore we may conclude, that theſe maxims will hold very nearly, when applied to works in large.

After the experiments above mentioned were tried, the wheel, which had originally 24 floats, was reduced to twelve; which cauſed a diminution in the effect, on account of a greater quantity of water eſcaping between the floats and the floor; but a circular ſweep being adapted thereto, of ſuch a length, that one float entered the curve before the preceding one quitted it, the effect came ſo near to the former, as not to give hopes of advancing it by increaſing the number of floats beyond 24 in this particular wheel.

PART II.

Concerning OVERSHOT WHEELS.

Read before the Royal Society, May, 24, 1759.

IN the former part of this eſſay, we have conſidered the impulſe of a confined ſtream, acting on *Underſhot Wheels.* We now proceed to examine the power and application of water, when acting by its *gravity* on *Overſhot Wheels.*

In reaſoning without experiment, one might be led to imagine, that however different the mode of application is; yet that whenever the ſame quantity of water deſcends through the ſame perpendicular ſpace, that the natural effective power would be equal; ſuppoſing the machinery free from friction, equally calculated to receive the full effect of the power, and to make the moſt of it: for if we ſuppoſe the height of a column of water to be 30 inches, and reſting upon a baſe or aperture of one inch ſquare, every cubic inch of water that departs therefrom will acquire the ſame velocity, or *momentum*, from the uniform preſſure of 30 cubic inches above it, that one cubic inch let fall from the top will acquire in falling down to the level of the aperture; *viz.* ſuch a velocity as, in a contrary direction, would carry it to the level from whence it fell; * one would therefore ſuppoſe, that a cubic inch of water, let fall through a ſpace of 30 inches, and there impinging upon another body, would be capable of producing an equal effect by colliſion, as if the ſame cubic inch had deſcended through the ſame ſpace with a ſlower motion, and produced its effects gradually: for in both caſes gravity acts upon an equal quantity of matter, through an equal ſpace;* and conſequently, that whatever was the ratio between the power and effect in underſhot wheels, the ſame would obtain in overſhot, and indeed in all others: yet, however concluſive this reaſoning may ſeem, it will appear, in the courſe of the following deductions, that the effect of the gravity of deſcending bodies is very different from the effect of the ſtroke of ſuch as are *non elaſtic*, though generated by an equal mechanical power.

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* This a conſequence of the riſing of jets to the height of their reſervoirs nearly.

The alterations in the machinery already deſcribed, to accommodate the ſame for experiments on overſhot wheels, were principally as follows:

PLATE II, *Fig.* 2. The ſluice I *b* being ſhut down, the rod H I was unſcrewed and taken off.

The underſhot water-wheel was taken off the axis, and inſtead thereof an overſhot wheel of the ſame diameter was put into its place. *Note*, This wheel was two inches in the ſhroud or depth of the bucket; the number of the buckets was 36.

The ſtandards S. and T, *Fig.* 1. were raiſed half an inch, ſo that the bottom of the wheel might be clear of ſtagnant water.

A trunk, for bringing the water upon the wheel, was fixed according to the dotted lines *f g*, *Fig.* 2. The aperture was adjuſted by a ſhuttle *h i*, which alſo cloſed up the outer end of the trunk, when the water was to be ſtopped.

Fig. 3. The ratchet *o e*, not being of one piece of metal with the ferrule *e e*, *i i* (though ſo deſcribed before, to prevent unneceſſary diſtinctions), was with its catch turned the contrary ſide; conſequently the moveable barrel would do its office equally, notwithſtanding the water-wheel, when at work, moved the contrary way.

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* Gravity, it is true, acts a longer ſpace of time upon the body that deſcends ſlow than upon that which falls quick; but this cannot occaſion the difference in the effect: for an elaſtic body falling through the ſame ſpace in the ſame time, will, by colliſion upon another elaſtic body, rebound nearly to the height from which it fell; or, by communicating its motion, cauſe an equal one to aſcend to the ſame height.

Specimen of a Set of Experiments.

Head 6 inches.

14 ½ ſtrokes of the pump in a minute, 12 ditto = 80 lb. *

Weight of the ſcale (being wet) 10½ oz.

Counterweight for 20 turns, beſides the ſcale, 3 oz.

No.	Weight in the Scale.	Turns.	Product.	Obſervations.
1	0 *lb.*	60	—	Threw moſt part of the water out of the wheel.
2	1	56	—	
3	2	52	—	
4	3	49	147	Received the water more quietly.
5	4	47	188	
6	5	45	225	
7	6	42½	255	
8	7	41	287	
9	8	38½	308	
10	9	36½	328½	
11	10	35½	355	
12	11	32¾	360½	
13	12	31¼	375	
14	13	28½	370½	
15	14	27½	385	
16	15	26	390	
17	16	24½	392	
18	17	22¾	386¾	
19	18	21¾	391½	
20	19	20¾	394¼	maximum.
21	20	19¾	395	
22	21	18¼	388¼	
23	22	18	396	Worked irregular.
24	23	Overſet by its load.		

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* The ſmall difference, in the value of 12 ſtrokes of the pump, from the former experiments, was owing to a ſmall difference in the length of the ſtroke, occaſioned by the warping of the wood.

Reduction of the preceding Specimen.

In theſe experiments the head being 6 inches, and the height of the wheel 24 inches, the whole deſcent will be 30 inches: the expence of water was 14 ½ ſtrokes of the pump in a minute, whereof 12 contained 80 lb.; therefore the water expended in a minute was 96 ⅔ lb. which, multiplied by 30 inches, gives the *power* = 2900.

If we take the 20th experiment for the *maximum*, we ſhall have 20 ¾ turns in a minute, each of which raiſed the weight 4 ½ inches, that is, 93,37 inches in a minute. The weight *in* the ſcale was 19 lb. the weight of the ſcale 10 ½ oz.; the counterweight 3 oz. in the ſcale, which, with the weight of the ſcale 10 ½ oz. makes in the whole 20½ lb. which is the whole reſiſtance or load: this, multiplied by 93,37 inches, makes 1914 for the effect.

The *ratio* therefore of the *power* and *effect* will be as 2900 : 1914, or as 10 : 6,6, or as 3 : 2 nearly.

But if we compute the power from the height of the wheel only, we ſhall have 96 ⅔ lb. multiplied by 24 inches = 2320 for the *power*, and this will be to the *effect* as 2320 : 1914, or as 10 : 82, or as 5 : 4 nearly.

The reduction of this ſpecimen is ſet down in N°. 9, of the following Table; and the reſt were deducted from a ſimilar ſet of experiments, reduced in the ſame manner.

TABLE

TABLE II. *containing the Reſult of Sixteen Sets of Experiments on Overſhot Wheels.*

N°.	Whole deſcent.	Water expended in a minute.	Turns at the maximum in a min.	Weight raiſed at the maximum.	Power of the whole deſcent.	Power of the wheel.	Effect.	Ratio of the whole power and effect.	Ratio of power of the wheel and effect.	Mean ratio.
	Inch.	*lb.*		*lb.*						
1	27	30	19	6½	810	720	556	10 : 6,9	10 : 7,7	Medium 10 : 8,1
2	27	56⅔	16¼	14½	1530	1360	1060	10 : 6,9	10 : 7,8	
3	27	56⅔	20¾	12½	1530	1360	1167	10 : 7,6	10 : 8,4	
4	27	63⅓	20½	13½	1710	1524	1245	10 : 7,3	10 : 8,2	
5	27	76⅔	21½	15½	2070	1840	1500	10 : 7,3	10 : 8,2	
6	28½	73⅓	18¾	17½	2090	1764	1476	10 : 7,	10 : 8,4	10 : 8,2
7	28½	96⅔	20¼	20½	2755	2320	1868	10 : 6,8	10 : 8,	
8	30	90	20	19½	2700	2160	1755	10 : 6,5	10 : 8,1	10 : 8,2
9	30	96⅔	20¾	20½	2900	2320	1914	10 : 6,6	10 : 8,2	
10	30	113⅓	21	23½	3400	2720	2221	10 : 6,5	10 : 8,2	
11	33	56⅔	20¼	13½	1870	1360	1230	10 : 6,6	10 : 9,	10 : 8,5
12	33	106⅔	22¼	21½	3520	2560	2153	10 : 6,1	10 : 8,4	
13	33	146⅔	23	27½	4840	3520	2846	10 : 5,9	10 : 8,1	
14	35	65	19¾	16½	2275	1560	1466	10 : 6,5	10 : 9,4	10 : 8,5
15	35	120	21½	25½	4200	2880	2467	10 : 5,9	10 : 8,6	
16	35	163½	25	26½	5728	3924	2981	10 : 5,2	10 : 7,6	
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11

Obſervations and Deductions from the foregoing Experiments.

I. *Concerning the Ratio between the Power and Effect of Overſhot Wheels.*

The effective power of the water muſt be reckoned upon the whole deſcent; becauſe it muſt be raiſed that height, in order to be in a condition of producing the ſame effect a ſecond time.

The ratios between the *powers* ſo eſtimated, and the *effects* at the *maximum* deduced from the ſeveral ſets of experiments, are exhibited at one view in column 9. of Table II.; and from hence it appears, that thoſe ratios differ from that of 10 to 7,6 to that of 10 : 5,2, that is, nearly from 4 : 3 to 4 : 2. In thoſe experiments where the heads of water and quantities expended are leaſt, the proportion is nearly as 4 : 3; but where the heads and quantities are greateſt, it approaches nearer to that of 4 : 2 ; and by a medium of the whole, the ratio is that of 3 : 2 nearly. We have ſeen before, in our obſervations upon the effects of underſhot wheels, that the general ratio of the power to the effect, when greateſt was 3 : 1 ; *the effect therefore of overſhot wheels, under the ſame circumſtances of quantity and fall, is at a medium double to that of the underſhot:* and, as a conſequence thereof, *that nonelaſtic bodies, when acting by their impulſe or colliſion, communicate only a part of their original power*; the other part being ſpent in changing their figure in conſequence of the ſtroke.

The powers of water computed from the height of the wheel only, compared with the effects, as in column 10, appear to obſerve a more conſtant ratio ; for if we take the medium of each claſs, which is ſet down in column 11, we ſhall find the extremes to differ no more than from the ratio of 10 : 8,1 to that of 10 : 8,5 ; and as the ſecond term of the ratio gradually increaſes from 8,1 to 8,5, by an increaſe of head from 3 inches to 11, the exceſs of 8,5 above 8,1 is, to be imputed to the ſuperior impulſe of the water at the head of 11 inches above that of 3 inches: ſo that if we reduce 8,1 to 8, on account of the impulſe of the 3 inch head, *we ſhall have the ratio of the power, computed upon the height of the wheel only, to the effect at a maximum as* 10 : 8, *or as* 5 : 4 *nearly*; and from the quality of the ratio between power and effect, ſubſiſting where the conſtructions are ſimilar, we muſt infer, *that the effects*, as well as the powers, *are as the quantities of water and perpendicular heights multiplied together reſpectively.*

II. *Con-*

II. *Concerning the moſt proper Height of the Wheel in proportion to the whole Deſcent.*

We have already ſeen, from the preceding obſervation, that the effect of the ſame quantity of water, deſcending through the ſame perpendicular ſpace, is double, when acting by its gravity upon an overſhot wheel, to what the ſame produces when acting by its impulſe upon an underſhot. It alſo appears, that by increaſing the head from 3 inches to 11, that is, the whole deſcent, from 27 inches to 35, or in the ratio of 7 to 9 nearly, the effect is advanced no more than in the ratio of 8,1 to 8,4, that is, as 7 : 7,26; and conſequently the increaſe of effect as not 1-7th of the increaſe of perpendicular height. Hence it follows, *that the higher the wheel is in proportion to the whole deſcent, the greater will be the effect*; becauſe it depends leſs upon the impulſe of the head, and more upon the gravity of the water in the buckets: and if we conſider how obliquely the water iſſuing from the head muſt ſtrike the buckets, we ſhall not be at a loſs to account for the little advantage that ariſes from the impulſe thereof: and ſhall immediately ſee of how little conſequence this impulſe is to the effect of an overſhot wheel. However, as every thing has its limits, ſo has this: for thus much is deſirable *that the water ſhould have ſomewhat greater velocity, than the circumference of the wheel, in coming thereon*; otherwiſe the wheel will not only be retarded, by the buckets ſtriking the water, but thereby daſhing a part of it over, ſo much of the power is loſt.

The velocity that the circumference of the wheel ought to have, being known by the following deductions, the head requiſite to give the water its proper velocity is eaſily computed from the common rules of hydroſtatics; and will be found much leſs than what is generally practiſed.

III. *Con-*

III. *Concerning the Velocity of the Circumference of the Wheel, in order to produce the greateſt Effect.*

If a body is let fall freely from the ſurface of the head to the bottom of the deſcent, it will take a certain time in falling; and in this caſe the whole action of gravity is ſpent in giving the body a certain velocity: but if this body in falling is made to act upon ſome other body, ſo as to produce a mechanical effect, the falling body will be retarded; becauſe a part of the action of gravity is then ſpent in producing the effect, and the remainder only giving motion to the falling body: and therefore *the ſlower a body deſcends, the greater will be the portion of the action of gravity applicable to the producing a mechanical effect*; and in conſequence the greater that effect may be.

If a ſtream of water falls into the bucket of an overſhot wheel, it is there retained until the wheel by moving round diſcharges it: of conſequence the ſlower the wheel moves, the more water each bucket will receive: ſo that what is loſt in ſpeed, is gained by the preſſure of a greater quantity of water acting in the buckets at once: and, if conſidered only in this light, the mechanical power of an overſhot wheel to produce effects will be equal whether it moves quick or ſlow: but if we attend to what has been juſt now obſerved of the falling body, it will appear that ſo much of the action of gravity, as is employed in giving the wheel and water therein a greater velocity, muſt be ſubtracted from its preſſure upon the buckets; ſo that, though the product made by multiplying the number of cubic inches of water acting in the wheel at once by its velocity will be the ſame in all caſes; yet, as each cubic inch, when the velocity is *greater* does not preſs ſo much upon the bucket as when it is *leſs*, the power of the water to produce effects will be greater in the leſs velocity than in the greater: and hence we are led to this general rule, *that*, cæteris paribus, *the leſs the velocity of the wheel, the greater will be the effect thereof.* A confirmation of this doctrine, together with the limits it is ſubject to in practice, may be deduced from the foregoing ſpecimen of a ſet of experiments.

From theſe experiments it appears that, when the wheel made about 20 turns in a minute, the effect was, near upon, the greateſt. When it made 30 turns, the effect was diminiſhed about $\frac{1}{20}$ part; but that when it made 40, it was diminiſhed about $\frac{1}{4}$; when it made leſs than 18$\frac{1}{4}$, its motion was irregular; and when it was loaded ſo as not to admit its making 18 turns, the wheel was overpowered by its load.

It is an advantage in practice, that the velocity of the wheel ſhould not be diminiſhed further than what will procure ſome ſolid advantage in point of power: becauſe, *cæteris paribus*, as the motion is ſlower, the buckets muſt be made larger; and the wheel being more loaded with water, the ſtreſs upon every part of the work will be increaſed in proportion: *The beſt velocity for practice therefore will be ſuch, as when the wheel here uſed made about 30 turns in a minute; that is, when the velocity of the circumference is a little more than 3 feet in a ſecond.*

Experience confirms, that this velocity of 3 feet in a ſecond is applicable to the higheſt overſhot wheels, as well as the loweſt; and all other parts of the work being properly adapted thereto, will produce very nearly the greateſt effect poſſible: however this alſo is certain from experience, that *high wheels may deviate further from this rule, before they will loſe their power, by a given aliquot part of the whole, than low ones can be admitted to do*; for a wheel of 24 feet high may move at the rate of ſix feet per ſecond without loſing any conſiderable part of its power *; and on the other hand, I have ſeen a wheel of 33 feet high, that has moved very ſteadily and well with a velocity but little exceeding 2 feet.

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* The 24 feet wheel going at 6 feet in a ſecond, ſeems owing to the ſmall proportion that the head (requiſite to give the water the proper velocity of the wheel) bears to the whole height.

IV. *Concerning the Load for an Overſhot Wheel, in order that it may produce a Maximum.*

The maximum load for an overſhot wheel, is that which reduces the circumferences of the wheel to its proper velocity; and this will be known, by dividing the effect it ought to produce in a given time by the ſpace intended to be deſcribed by the circumference of the wheel in the ſame time: the quotient will be the reſiſtance overcome at the circumference of the wheel; and is equal to the load required, the friction and reſiſtance of the machinery included.

V. *Concerning the greateſt poſſible Velocity of an Overſhot Wheel.*

The greateſt velocity that the circumference of an overſhot wheel is capable of, depends jointly upon the diameter or *height* of the wheel, and the velocity of falling bodies; for it is plain that the velocity of the circumference can never be greater, than to deſcribe a ſemi-circumference, while a body let fall from the top of the wheel will deſcend through its diameter; nor indeed quite ſo great, as a body deſcending through the ſame perpendicular ſpace cannot perform the ſame in ſo ſmall a time when paſſing through a ſemi-circle, as would be done in a perpendicular line. Thus, if a wheel is 16 feet 1 inch high, a body will fall through the diameter in one ſecond: this wheel therefore can never arrive at a velocity equal to the making one turn in two ſeconds; but, in reality, an overſhot wheel can never come near this velocity; for when it acquires a certain ſpeed, the greateſt part of the water is prevented from entering the buckets; and the reſt, at a certain point of its deſcent, is thrown out again by the centrifugal force. This appears to have been the caſe in the three firſt experiments of the foregoing ſpecimen; but as the velocity, when this begins to happen, depends upon the form of the buckets, as well as other circumſtances, *the utmoſt velocity of overſhot wheels is not to be determined generally*: and, indeed, it is the leſs neceſſary in practice, as it is in this circumſtance incapable of producing any *mechanical effect*, for reaſons already given.

VI. *Concerning the greateſt Load that an Overſhot Wheel can overcome.*

The greateſt load an overſhot wheel will overcome, conſidered abſtractedly, is unlimited or infinite: for as the buckets may be of any given capacity, the more the wheel is loaded, the ſlower it turns; but the ſlower it turns, the more will the buckets be filled with water; and conſequently though the diameter of the wheel and quantity of water expended, are both limited, yet no reſiſtance can be aſſigned, which it is not able to overcome: but in practice we always meet with ſomething that prevents our getting into infiniteſimals; for when we really go to work to build a wheel, the buckets muſt neceſſarily be of ſome given capacity; and conſequently *ſuch a reſiſtance will ſtop the wheel, as is equal to the effort of all the buckets in one ſemi-circumference filled with water.*

The ſtructure of the buckets being given; the quantity of this effort may be aſſigned; but is not of much conſequence to the practice, as in this caſe alſo the wheel loſes its power; for though here be the exertion of gravity upon a given quantity of water, yet being prevented by a counterbalance from moving, is capable of producing no *mechanical effect*, according to our definition. But, in reality, an overſhot wheel generally ceaſes to be uſeful before it is loaded to that pitch; for *when it meets with* *ſuch a reſiſtance as to diminiſh its velocity to a certain degree, its motion becomes irregular*; *yet this never happens until the velocity of the circumference is leſs than 2 feet per ſecond, where the reſiſtance is equable*, as appears not only from the preceding ſpecimen, but from experiments on larger wheels.

SCHOLIUM.

Having now examined the different effects of the power of water, when acting by its *impulſe*, and by its *weight*, under the titles of *underſhot* and *overſhot* wheels; we might naturally proceed to examine the effects when the impulſe and weight are combined, as in the ſeveral kinds of *breaſt wheels*, *&c.* but, what has been already delivered being carefully attended to, the application of the ſame principles in theſe mixt caſes will be eaſy, and reduce what I have to ſay on this head into a narrow compaſs: for all kinds of wheels where the water cannot deſcend through a given ſpace, unleſs the wheel moves therewith, are to be conſidered of the nature of an overſhot wheel, according to the perpendicular height that the water deſcends from; and all thoſe that receive the impulſe or ſhock of the water, whether in an horizontal, perpendicular, or oblique direction, are to be conſidered as underſhots. And therefore a wheel, which the water ſtrikes at a certain point below the ſurface of the head, and after that deſcends in the arch of a circle, preſſing by its gravity upon the wheel; *the effect of ſuch a wheel will be equal to the effect of an underſhot, whoſe head is equal to the difference of level between the ſurface of the water in the reſervoir, and the point where it ſtrikes the wheel, added to that of an overſhot, whoſe height is equal to the difference of level, between the point where it ſtrikes the wheel and the level of the tail-water.* It is here ſuppoſed, that the wheel receives the ſhock of the water at right angles to its radii; and that the velocity of its circumference is properly adapted to receive the utmoſt advantage of both theſe powers; otherwiſe a reduction muſt be made on that account.

Many obvious and conſiderable improvements upon the common practice naturally offer themſelves, from a due conſideration of the principles here eſtabliſhed, as well as many popular errors ſhow themſelves in view: but as my preſent purpoſe extends no farther than the laying down ſuch general rules as will be found to anſwer in practice, I leave the particular application to the intelligent artiſt, and to the curious in theſe matters.

PART III.

On the CONSTRUCTION *and* EFFECTS *of* WINDMILL SAILS.

Read before the Royal Society, 31 *May and* 14 *June,* 1759.

IN trying experiments on wind mill ſails, the wind itſelf is too uncertain to anſwer the purpoſe; we muſt therefore have recourſe to on artificial wind.

This may be done two ways; either by cauſing the air to move againſt the machine, or the machine to move againſt the air. To cauſe the air to move againſt the machine, in a ſufficient volume, with ſteadineſs and the requiſite velocity, is not eaſily put in practice: To carry the machine forward in a right line againſt the air, would require a larger room than I could conveniently meet with. What I found moſt practicable, therefore, was to carry the axis, whereon the ſails were to be fixed, progreſſively round in the circumference of a large circle. Upon this idea * a machine was conſtructed, as follows.

PLATE

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* Some years ago Mr. Rouſe, an ingenious gentleman of Harborough, in Leiceſterſhire, ſet about trying experiments on the velocity of the wind, and force thereof upon plain ſurfaces and windmill-ſails: and, much about the ſame time, Mr. Ellicott contrived a machine for the uſe of the late celebrated Mr. B. Robins, for trying the reſiſtance of plain ſurfaces moving through the air. The machines of both theſe gentleman were much alike, though at that time totally unacquainted with each other's inquiries. But it often happens, that when two perſons think juſtly upon the ſame ſubject, their experiments are alike. This machine was alſo built upon the ſame idea as the foregoing; but differed in having the hand for the firſt mover, with a pendulum for its regulator, inſtead of a weight,

as

PLATE III. Fig. 1.

A B C is a pyramidical frame for ſupporting the moving parts.

D E is an upright axis, whereon is framed

F G, an arm for carrying the ſails at a proper diſtance from the centre of the upright axis.

H is a barrel upon the upright axis, whereon is wound a cord; which, being drawn by the hand, gives a circular motion to the axis, and to the arm F G; and thereby carries the axis of the ſails in the circumference of a circle, whoſe radius is D I, cauſing thereby the ſails to ſtrike the air, and turn round upon their own axis.

At L is fixed the end of a ſmall line, which paſſing through the pullies M N O, terminates upon a ſmall cylinder or barrel upon the axis of the ſails, and, by winding thereon raiſes

P the ſcale, wherein the weights are placed for trying the power of the ſails. This ſcale, moving up and down in the direction of the upright axis, receives no diſturbance from the circular motion.

Q R two parallel pillars ſtanding upon the arm F G, for the purpoſe of ſupporting and keeping ſteady the ſcale P; which is kept from ſwinging by means of

as in the former; which was certainly beſt for the purpoſes of meaſuring the impulſe of the wind, or reſiſtance of plains: but the latter is more applicable to experiments on windmill-ſails; becauſe every change of poſition of the ſame ſails will occaſion their meeting the air with a different velocity, though urged by the ſame weight.

S T, two ſmall chains, which hang looſely round the two pillars.

W is a weight for bringing the centre of gravity of the moveable part of the machine into the centre of motion of the axis D E.

V X is a pendulum, compoſed of two balls of lead, which are moveable upon a wooden rod, and thereby can be ſo adjuſted, as to vibrate in any time required. This pendulum hangs upon a cylindrical wire, whereon it vibrates, as on a rolling axis.

Y is a perforated table for ſupporting the axis of the pendulum.

Note, The pendulum being ſo adjuſted, as to make two vibrations in the time that the arm F G is intended to make one turn; the pendulum being ſet a vibrating, the experimenter pulls by the cord Z, with ſufficient force to make each half revolution of the arm to correſpond with each vibration, as equal as poſſible, during the number of vibrations that the experiment is intended to be continued. A little practice renders it eaſy to give motion thereto with all the regularity that is neceſſary.

Specimen of a Set of Experiments.

Radius of the ſails	21 inches
Length of ditto in the cloth	18
Breadth of ditto	5,6
* { Angle at the extremity	10 degrees
{ Ditto at the greateſt inclination	25.

20

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* In all the following experiments the angle of the ſails is accounted from the plain of their motion; that is, when they ſtand

at

20 turns of the ſails raiſed the weight — 11,3 inches.

Velocity of the centre of the ſails, in the circumference of the great circle, in a ſecond — — — } 6 feet 0 inches

Continuance of the experiment — 52 ſeconds.

No.	Wt. in the ſcale.	Turns.	Product.
1	0*lb.*	108	0
2	6	85	510
3	6½	81	526½
4	7	78	546
5	7½	73	547½ maximum.
6	8	65	520
7	9	0	0

N. B. The weight of the ſcale and pulley was 3oz.; and that 1oz. ſuſpended upon one of the radii, at 12⅓ inches from the centre of the axis, juſt overcame the friction ſcale and load of 7½lb.; and placed at 14$\frac{19}{20}$ inches, overcame the ſame reſiſtances with 9 lb. in the ſcale.

Reduction of the preceding Specimen.

N°. 5. being taken for the maximum, the weight in the ſcale was 7lb. 8oz. which, with the weight of the ſcale and pulley 3oz. makes 7lb. 11oz. equal to 123oz.; this added to the friction of the machinery, the ſum is the whole reſiſtance*. The

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at right angles to the axis, their angle is denoted 0°, this notation being agreeable to the language of practitioners, who call the angle ſo denoted, the weather of the ſail; which they denominate greater or leſs, according to the quantity of this angle.

* The reſiſtance of the air is not taken into the account of reſiſtance, becauſe it is inſeparable from the application of the power.

friction of the machinery is thus deduced: Since 20 turns of the ſails raiſed the weight 11,3 inches, with a double line, the radius of the cylinder will be .18 of an inch; but had the weight been raiſed by a ſingle line, the radius of the cylinder being half the former, *viz.* .09, the reſiſtance would have been the ſame: we ſhall therefore have this analogy; as half the radius of the cylinder, is to the length of the arm where the ſmall weight was applied; ſo is the weight applied to the arm, to a fourth weight, which is equivalent to the ſum of the whole reſiſtance together; that is, .09 : 12,5 : : 1 oz. : 139 oz. this exceeds 123 oz. the weight in the ſcale, by 16 oz. or 1 lb. which is equivalent to the friction; and which, added to the above weight of 7 lb. 11 oz. makes 8 lb. 11 oz. = 8,69 lb. for the ſum of the whole reſiſtance; and this, multiplied by 73 turns, makes a product of 634, which may be called the repreſentative of the *effect* produced.

In like manner, if the weight 9 lb. which cauſed the ſails to reſt after being in motion, be augmented by the weight of the ſcale and its relative friction, it will become 10,37 lb. The reſult of this ſpecimen is ſet down in N°. 12. of Table III. and the reſult of every other ſet of experiments therein contained were made and reduced in the ſame manner.

TABLE

TABLE III. *Containing Nineteen Setts of Experiments on Windmill-Sails of various Structures, Poſitions, and Quantities of Surfaces.*

The kind of ſails made uſe of	N°.	Angle at the extremities	Greateſt angle.	Turns of the ſails unloaded.	Turns of ditto at the maximum.	Load at the maximum.	Greateſt load	Product.	Quantity of ſurface.	Ratio of greateſt velocity to the velocity at maxim^m.	Ratio of greateſt load to the load at maximum.	Ratio of ſurface to the product.
		°	°			*lb.*	*lb.*		*Sq.In*			
Plain ſails at an angle of 55°	1	35	35	66	42	7,56	12,59	318	404	10:7	10:6	10:7,9
Plain ſails weather'd according to the common practice.	2	12	12		70	6,3	7,56	441	404		10:8,3	10:10,1
	3	15	15	105	69	6,72	8,12	464	404	10:6,6	10:83	10:10,15
	4	18	18	96	66	7,0	9,81	462	404	10:7,	10:7,1	10:10,15
Weathered according to *Maclaurin's theorem.*	5	9	26½		66	7,0		462	404			10:11,4
	6	12	29½		70½	7,35		518	404			10:12,8
	7	15	32½		63½	8,3		527	404			10:13,
Sails weathered in the *Dutch* manner, tried in various poſitions.	8	0	15	120	93	4,75	5,31	442	404	10:7,7	10:8,9	10:11,
	9	3	18	120	79	7,0	8,12	553	404	10:6,6	10:8,6	10:13,7
	10	5	20		78	7,5	8,12	585	404		10:9,2	10:14,5
	11	7½	22½	113	77	8,3	9,81	639	404	10:6,8	10:8,5	10:15,8
	12	10	25	108	73	8,69	10,37	634	404	10:6,8	10:8,4	10:15,7
	13	12	27	100	66	8,41	10,94	580	404	10:6,6	10:7,7	10:14,4
Sails weathered in the *Dutch* manner, but *enlarged* towards the extremities.	14	7½	22½	123	75	10,65	12,59	799	505	10:6,1	10:8,5	10:15,8
	15	10	25	117	74	11,08	13,69	820	505	10:6,3	10:8,1	10:16,2
	16	12	27	114	66	12,09	14,23	799	505	10:5,8	10:8,4	10:15,8
	17	15	30	96	63	12,09	14,78	762	505	10:6,6	10:8,2	10:15,1
8 ſails being *ſectors* of *ellipſes* in their beſt poſitions.	18	12	22	105	64½	16,42	27,87	1059	854	10:6,1	10:5,9	10:12,4
	19	12	22	99	64½	18,06		1165	1146	10:5[illegible]		10:10,1
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.

To face page 43.

Obſervations and Deductions from the preceding Experiments.

I. *Concerning the beſt Form and Poſition of Windmill-Sails.*

In Table III. N°. 1. is contained the reſult of a ſet of experiments upon ſails ſet at the angle which the celebrated Monſ. Parint, and ſucceeding geometricians for many years, held to be the beſt; *viz.* thoſe whoſe planes make an angle 55° nearly with the axis; the complement whereof, or angle that the plane of the ſail makes with the plane of their motion, will therefore be 35°, as ſet down in col. 2. and 3. Now if we multiply their number of turns by the weight they lifted, when working to the greateſt advantage, as ſet down in columns 5. and 6. and compare this product (col. 8.) with the other products contained in the ſame column, inſtead of being the greateſt, it turns out the leaſt of all the reſt. But if we ſet the angle of the ſame planes at ſomewhat leſs than half the former, or at any angle from 15° to 18°, as in N°. 3. and 4. that is, from 72° to 75° with the axis, the product will be increaſed in the ratio of 31 : 45; and this is the angle moſt commonly made uſe of by practitioners, when the ſurfaces of the ſails are planes.

If nothing more was intended than to determine the moſt efficacious angle to make a mill acquire motion from a ſtate of reſt, or to prevent it from paſſing into reſt from a ſtate of motion, we ſhall find the poſition of N°. 1. the beſt; for if we conſult col. 7. which contains the leaſt weights, that would make the ſails paſs from motion to reſt, we ſhall find that of N°. 1. (relative to the quantity of cloth) the greateſt of all. But if the ſails are intended, with given dimenſions, to produce the greateſt effect poſſible in a given time, we muſt entirely reject thoſe of

N°.

N°. 1. and, *if we are confined to the uſe of planes, conform ourſelves to ſome angle between* N°. 3. *and* 4. *that is, not leſs than* 72°, *or greater than* 75°, *with the axis.*

The late celebrated Mr. Maclaurin has judiciouſly diſtinguiſhed between the action of the wind upon a ſail at reſt, and a ſail in motion; and, in conſequence, as the motion is more rapid near the extremities than towards the centre, that the angle of the different parts of the ſail, as they recede from the centre, ſhould be varied. For this purpoſe he has furniſhed us with the following theorem *. "Suppoſe the velocity of the wind to "be repreſented by *a*, and the velocity of any given part of the "ſail to be denoted by *c*; then the effort of the wind upon that "part of the ſail will be greateſt when the tangent of the angle, "in which the wind ſtrikes it, is to radius as "$\sqrt{2+\frac{9cc}{4aa}}+\frac{3c}{2a}$ to 1." This theorem then aſſigns the law, by which the angle is to be varied according to the velocity of each part of the ſail to the wind: but as it is left undetermined what velocity any one given part of the ſail ought to have in reſpect to the wind, the angle that any one part of the ſail ought to have, is left undetermined alſo; ſo that we are ſtill at a loſs for the proper *data* to apply the theorem. However, being willing to avail myſelf thereof, and conſidering that any angle from 15° to 18° was beſt ſuited to a plane, and of conſequence to the beſt mean angle, I made the ſail, at the middle diſtance between the centre and the extremity, to ſtand at an angle of 15° 41′ with the plane of the motion; in which caſe the velocity of that part of the ſail, when loaded to a *maximum*, would be equal to that of the wind, or $c = a$. This being determined, the reſt were inclined according to the theorem, as follows:

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* Maclaurin's account of Sir Iſaac Newton's philoſophical diſcoveries, p. 176, art. 29.

Angle

Parts of the radius from the centre.		Angle with the axis.	Angle of weather.	
$\frac{1}{6}$	$c=\frac{1}{3}a$	63° 26′	26° 34′	
$\frac{2}{6}$	$c=\frac{2}{3}a$	69 54	20 6	
$\frac{1}{2}$	$c=a$	74 19	15 41	middle.
$\frac{2}{3}$	$c=1\frac{1}{3}a$	77 20	12 40	
$\frac{5}{6}$	$c=1\frac{2}{3}a$	79 27	10 33	
1	$c=2a$	81 0	9 0	extremity.

The reſult hereof was according to N°. 5. being nearly the ſame as the plane ſails, in their beſt poſition: but being turned round in their ſockets, ſo that every part of each ſail ſtood at an angle of 3°, and afterwards of 6°, greater than before, that is, their extremities being moved from 9° to 12° and 15°, the products were advanced to 518 and 527 reſpectively. Now from the ſmall difference between thoſe two products, we may conclude, that they were nearly in their beſt poſition, according to N°. 7. or ſome angle between that and N°. 6: but from theſe, as well as the plane ſails and others, we may alſo conclude, that *a variation in the angle of a degree or two makes very little difference in the effect, when the angle is near upon the beſt.*

It is to be obſerved, that a ſail inclined by the preceding rule will expoſe a convex ſurface to the wind: whereas the Dutch, and all our modern mill-builders, though they make the angle to diminiſh, in receding from the centre towards the extremity, yet conſtantly do it in ſuch manner, as that the ſurface of the ſail may be concave towards the wind. In this manner the ſails made uſe of in N°. 8, 9, 10, 11, 12, and 13, were conſtructed; the middle of the ſail making an angle with the extreme bar of 12°; and the greateſt angle (which was about $\frac{1}{3}$ of the radius from the centre) of 15° therewith. Thoſe ſails being tried in various poſitions, the beſt appears to be that of N°. 11. where the extremities ſtood at an angle of 7°$\frac{1}{2}$ with the plane of motion, the product being 639: greater than that of thoſe made by the theorem in the ratio of 9 : 11, and double to that of N°. 1; and this was the greateſt product that could be procured without an augmentation of ſurface. Hence it appears, that *when the wind falls upon a concave ſurface, it is an advantage to the power of the whole, though every part, taken ſeparately, ſhould not be diſpoſed to the beſt advantage* *.

Having thus obtained the beſt poſition of the ſails, or manner of weathering, as it is called by the workmen, the next point was to try what advantage could be made by an addition of ſurface upon the ſame radius. For this purpoſe, the ſails made uſe of had the ſame weather as thoſe N°. 8. to 13, with an addition to the leading ſide of each of a triangular cloth, whoſe height was equal to the height of the ſail, and whoſe baſe was equal to half the breadth: of conſequence the increaſe of ſurface upon the whole was one-fourth part, or as 4 : 5. Thoſe ſails, by being turned round in their ſockets, were tried in four different poſitions, ſpecified in N°. 14, 15, 16, and 17; from whence it appears, that the beſt was when every part of the ſail made a greater angle by 2°½, with the plane of the motion, than thoſe without the addition, as appears by N°. 15. the product being

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* By ſeveral trials in large I have found the following angles to anſwer as well as any. The radius is ſuppoſed to be divided into 6 parts and 1-6th, reckoning from the centre, is called 1, the extremity being denoted 6.

N°.	Angle with the axis.	Angle with the plane of the motion.
1	72°	18°
2	71	19
3	72	18 middle.
4	74	16
5	77½	12½
6	83	7 extremity.

820:

820: this exceeds 639 more than in the ratio of 4 : 5, or that of the increaſe of cloth. Hence it appears, that *a broader ſail requires a greater angle*; and *that when the ſail is broader at the extremity, than near the centre, this ſhape is more advantageous than that of a parallelogram**.

Many have imagined, that the more ſail, the greater the advantage, and have therefore propoſed to fill up the whole area: and by making each ſail a ſector of an ellipſis, according to Monſieur Parint, to intercept the whole cylinder of wind, and thereby to produce the greateſt effect poſſible.

We have therefore proceeded to inquire how far the effect could be increaſed by a further enlargement of the ſurface, upon the ſame radius of which N°. 18 and 19 are ſpecimens. The ſurfaces indeed were not made planes, and ſet at an angle of 35°, as Parint propoſed; becauſe, from N°. 1. we learn, that this poſition has nothing to do, when we intend them to work to the greateſt advantage. We therefore gave them ſuch an angle as the preceding experiments indicated for ſuch ſort of ſails, *viz.* 12° at the extremity, and 22° for the greateſt weather. By N°. 18 we have the product 1059, greater than N°. 15. in the ratio of 7 : 9; but then the augmentation of cloth is almoſt 7 : 12. By N°. 19. we have the product 1165, that is greater than N°. 15. as 7 : 10; but the augmentation of cloth is nearly as 7 : 16; conſequently had the ſame quantity of cloth as in

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* The figure and proportion of the enlarged ſails, which I have found beſt to anſwer in large, are repreſented in the figure, Plate III. where the extreme bar is 1-3d of the radius (or whip, as it is called by the workmen), and is divided by the whip in the proportion of 3 to 5. The triangular or leading ſail is covered with board from the point downwards 1-3d of its height, the reſt with cloth as uſual. The angles of weather in the preceding note are beſt for the enlarged ſails alſo; for in practice it is found, that the ſails had better have too little than too much weather.

N°.

N°. 18. been diſpoſed in a figure ſimilar to that of N°. 15, inſtead of the product 1059, we ſhould have had the product 1386; and in N°. 19, inſtead of the product 1165, we ſhould have had a product of 1860; as will be further made appear in the courſe of the following deductions. Hence it appears, that beyond a certain degree, the more the area is crouded with ſail, the leſs effect is produced in proportion to the ſurface: and, by purſuing the experiments ſtill further, I found, that though in N°. 19. the ſurface of all the ſails together were not more than 7-8ths of the circular area containing them, yet a further addition rather diminiſhed than increaſed the effect. *So that when the whole cylinder of wind is intercepted, it does not then produce the greateſt effect for want of proper interſtices to eſcape.*

It is certainly deſirable that the ſails of windmills ſhould be as ſhort as poſſible; but at the ſame time it is equally deſirable, the quantity of cloth ſhould be the leaſt that may be, to avoid damage by ſudden ſqualls of wind. The beſt ſtructure, therefore, for large mills, is that where the quantity of cloth is the greateſt, in a given circle, that can be: on this condition, that the effect holds out in proportion to the quantity of cloth; for otherwiſe the effect can be augmented in a given degree by a leſſer increaſe of cloth upon a larger radius, than would be required, if the cloth was increaſed upon the ſame radius. The moſt uſeful figure therefore for practice, is that of N°. 9. or 10. as has been experienced upon ſeveral mills in large.

TABLE IV. *Containing the Reſult of ſix Sets of Experiments, made for determining the Difference of Effect, according to the different Velocity of the Wind.*

N. B. The ſails were of the ſame ſize and kind as thoſe of N° 10, 11, and 12, Tab. III. Continuance of the Experiment one minute.

N°.	Angle at the extremity.	Velocity of the wind in a ſecond.	Turns of the ſails unloaded.	Turns of the ſails at maximum.	Load at the maximum.	Greateſt load.	Product.	Maximum load for the half velocity.	Turns of the ſails therewith	Product of leſſer load and greater velocity.	Ratio of the two products.	Ratio of the greateſt velocity to the velocity at a maximᵐ.	Ratio of the greateſt load to the load at a maximum.
		f. in.			*lb.*	*lb.*							
1	5°	4 4½	96	66	4,47	5,37	295	—	—	—	— —	10 ; 6,9	10 : 8,3
2	5	8 9	207	122	16,42	18,06	2003	4,47	180	805	10 : 27,3	10 : 5,9	10 : 9,1
3	7½	4 4½	—	65	4,6	—	300	—	—	—	— —	—	—
4	7½	8 9	—	130	17,52	—	2278	4,62	180	832	10 : 27,8	—	—
5	10	4 4½	91	61	5,03	5,87	307	—	—	—	— —	10 ; 6,7	10 : 8,5
6	10	8 9	178	110	18,61	21,34	2047	5,03	158	795	10 : 26	10 : 6,2	10 : 8,7
1	2	3	4	5	6	7	8	9	10	11	12	13	14

II. *Concerning the ratio between the velocity of windmill ſails unloaded, and their velocity when loaded to a maximum.*

Thoſe ratios, as they turned out in experiments upon different kinds of ſails, and with different inclinations (the velocity of the wind being the ſame) are contained in column 10 of tab. III. where the extremes differ from the ratio of 10 : 7,7 to that of 10 : 5,8; but *the most general ratio of the whole will be nearly as* 3 : 2. This ratio alſo agrees ſufficiently near with experiments where the velocity of the wind was different, as in thoſe contained in tab. IV. col. 13. in which the ratios differ from 10 : 6,9 to that of 10 : 5,9. However, it appears in general, that where the power is greater, whether by an enlargement of ſurface, or a greater velocity of the wind, that the ſecond term of the ratio is leſs.

III. *Concerning the ratio between the greateſt load that the ſails will bear without ſtopping, or what is nearly the ſame thing, between the leaſt load that will ſtop the ſails, and the load at the maximum.*

Thoſe ratios for different kinds of ſails and inclinations, are collected in col. 11. tab. III. where the extremes differ from the ratio of 10 : 6 to that of 10 : 9,2; but taking in thoſe ſets of experiments only, where the ſails reſpectively anſwered beſt, *the ratio's will be confined between that of* 10 : 8 *and of* 10 : 9; *and at a medium about* 10 : 8,3 *or of* 6 : 5. This ratio alſo agrees nearly with thoſe in col. 14 of tab. IV. However it appears, upon the whole, that in thoſe inſtances, where the angle of the ſails or quantity of cloth were greateſt, that the ſecond term of the ratio was leſs.

IV. *Concerning the effects of ſails, according to the different velocity of the wind.*

Maxim 1. *The velocity of windmill ſails, whether unloaded, or loaded, ſo as to produce a maximum, is nearly as the velocity of the wind, their ſhape and poſition being the ſame.*

This appears by comparing together the reſpective numbers of columns 4 and 5, tab. IV. wherein thoſe of numbers 2, 4, and 6, ought to be double of numbers 1, 3, and 5: but as the deviation is no where greater than what may be imputed to the inaccuracy of the experiments themſelves, and hold good exactly in numbers 3 and 4; which ſets were deduced from the medium of a number of experiments, carefully repeated the ſame day, and on that account are moſt to be depended upon; we may therefore conclude the maxim true.

Maxim 2d. *The load at the maximum is nearly, but ſomewhat leſs than, as the ſquare of the velocity of the wind, the ſhape and poſition of the ſails being the ſame.*

This appears by comparing together the numbers in col. 6. tab. IV. wherein thoſe of numbers 2, 4, and 6 (as the velocity is double), ought to be quadruple of thoſe of numbers 1, 3, and 5; inſtead of which they fall ſhort, number 2 by $\frac{1}{14}$, number 4 by $\frac{1}{19}$, and number 6 by $\frac{1}{13}$ part of the whole. The greateſt of thoſe deviations is not more conſiderable than might be imputed to the unavoidable errors in making the experiments: but as thoſe experiments, as well as thoſe of the greateſt load, all deviate the ſame way; and alſo coincide with ſome experiments communicated to me by Mr. Rouſe upon the reſiſtance of planes; I am led to ſuppoſe a ſmall deviation, whereby the load falls ſhort of the ſquares of the velocity; and ſince the experiments N°. 3 and 4, are moſt to be depended upon, we muſt conclude, that when the velocity is double, the load falls ſhort of its due Proportion by $\frac{1}{19}$, or, for the ſake of a round number, by about $\frac{1}{20}$ part of the whole.

Maxim 3d. *The effects of the ſame ſails at a maximum are nearly, but ſomewhat leſs than, as the cubes of the velocity of the wind.*

It has already been proved, Maxim 1ſt, that the velocity of ſails at the *maximum*, is nearly as the velocity of the wind; and by Maxim 2d, that the load at the *maximum* is nearly as the ſquare of the ſame velocity: if thoſe two maximums would hold preciſely, it would be a conſequence that the effect would be in a triplicate ratio thereof: how this agrees with experiment will appear by comparing together the products in col. 8 of tab. 4, wherein thoſe of N°. 2, 4, and 6, (the velocity of the wind being double) ought to be octuple of thoſe of N°. 1, 3, and 5, inſtead of which they fall ſhort, N°. 2, by $\frac{1}{7}$ N°. 4 by $\frac{1}{20}$, and N°. 6, by $\frac{1}{6}$ part of the whole. Now, if we rely on N°. 3, and 4, as the turns of the ſails are as the velocity of the wind; and ſince the load of the maximum falls ſhort of the ſquare of the velocity by about $\frac{1}{20}$ part of the whole: the product made by the multiplication of the turns into the load, muſt alſo fall ſhort of the triplicate ratio by about $\frac{1}{20}$ part of the whole product.

Maxim 4th. *The load of the ſame ſails at the maximum is nearly as the ſquares, and their effect as the cubes of their number of turns in a given time.*

This maxim may be eſteemed a conſequence of the three preceding; for if the turns of the ſails are as the velocity of the wind, whatever quanties are in any given ratio of the velocity of the wind, will be in the ſame given ratio of the turns of the ſails: and therefore, if the load at the *maximum* is as the ſquare, or the effect as the cube, of the velocity of the wind, wanting $\frac{1}{20}$

part when the velocity is double; the load at the *maximum* will alſo be as the ſquare, and the effect as the cube, of the number of turns of the ſails in a given time, wanting in like manner $\frac{1}{20}$ part when the number of turns are double in the ſame time. In the preſent caſe, if we compare the loads at the *maximum*, col.6. with the ſquares of the number of turns, col. 5. of N°. 1 and 2, 5 and 6, or the products of the ſame numbers col. 8, with the cubes of the number of turns col 5, inſtead of falling ſhort, as N°.3, and 4, they exceed thoſe ratios: but as the ſets of experiments N°.1 and 2, of 5 and 6,are not to be eſteemed of equal authority with thoſe of N°. 3 and 4, we muſt not rely upon them further than to obſerve, that *in comparing the groſs effects of large machines, the direct proportion of the ſquares and cubes reſpectively, will hold as near as the effects themſelves can be obſerved*; and therefore be ſufficient for practical eſtimation, without any allowance.

Maxim 5th. *When ſails are loaded ſo as to produce a maximum at a given velocity, and the velocity of the wind increaſes, the load continuing the ſame; 1ſtly, The increaſe of effect, when the increaſe of the velocity of the wind is ſmall, will be nearly as the ſquares of thoſe velocities: 2dly, When the velocity of the wind is double, the effects will be nearly as 10 : 27½: But, 3dly, When the velocities compared, are more than double of that where the given load produces a maximum, the effects increaſe nearly in a ſimple ratio of the velocity of the wind.*

It has already been proved, maxim 1ſt and 2d, that when the velocity of the wind is increaſed, the turns of the ſails will increaſe in the ſame proportion, even when oppoſed by a load as the ſquare of the velocity: and therefore if wanting the oppoſition of an increaſe of load, as the ſquare of the velocity, the turns of the ſails will again be increaſed in a ſimple ratio of the velocity of the wind on that account alſo; that is, the load continuing the ſame, the turns of the ſails in a given time will be as the ſquare of the velocity of the wind; and the effect, being in this caſe as the turns of the ſails will be as the ſquare of the velocity of the wind alſo; but this muſt be underſtood only of the firſt increments of the velocity of the wind: for,

2dly, As the ſails will never acquire above a given velocity in relation to the wind, though the load was diminiſhed to nothing; when the load continues the ſame, the more the velocity of the wind increaſes (though the effect will continue to increaſe) yet the more it will fall ſhort of the ſquare of the velocity of the wind; ſo that when the velocity of the wind is double, the increaſe of effect, inſtead of being as 1 : 4, according to the ſquares, it turns out as 10 : 27½, as thus appears. In tab. 4. col. 9. the loads of N°. 2, 4, and 6, are the ſame as the maximum loads in col. 6. of N°. 1, 3, and 5. The number of turns of the ſails with thoſe loads, when the velocity of the wind is double, are ſet down in col. 10, and the products of their multiplication in col. 11: thoſe being compared with the products of N°. 1, 3, and 5, col. 8, furniſh the ratios ſet down in col. 12, which at a medium (due regard being had to N°. 3 and 4.) will be nearly as 10 : 27½. 3dly. The load continuing the ſame, grows more and more inconſiderable, reſpecting the power of the wind as it increaſes in velocity; ſo that the turns of the ſails grow nearer and nearer a coincidence with their turns unloaded; that is, nearer and nearer to the ſimple ratio of the velocity of the wind. When the velocity of the wind is double, the turns of the ſails, when loaded to a maximum, will be double alſo; but, *unloaded*, will be no more than triple, by deduction 2d: and therefore the product could not have increaſed beyond the ratio of 10 : 30 (inſtead of 10 : 27½) even ſuppoſing the ſails not to have been retarded at all by carrying the maximum load for the half velocity. Hence we ſee, that when the velocity of the wind exceeds the double of that, where a conſtant load produces a maximum, that the increaſe of effect, which follows the increaſe of the velocity of the ſails, will be nearly as the velocity of the wind, and ultimately in that ratio preciſely. Hence alſo we ſee that windmills, ſuch as the different ſpecies for raiſing water for drainage, &c. loſe much of their full effect, when acting againſt one invariable oppoſition.

V. *Concerning the effects of ſails of different magnitudes the ſtructure and poſition being ſimilar, and the velocity of the wind the ſame.*

Maxim 6. *In ſails of a ſimilar figure and poſition, the number of turns in a given time will be reciprocally as the radius or length of the ſail.*

The extreme bar having the ſame inclination to the plain of its motion, and to the wind; its velocity at a *maximum* will always be in a given ratio to the velocity of the wind; and therefore, whatever be the radius, the abſolute velocity of the extremity of the ſail will be the ſame: and this will hold good reſpecting any other bar, whoſe inclination is the ſame, at a proportionable diſtance from the center; it therefore follows, that the extremity of all ſimilar ſails, with the ſame wind, will have the ſame abſolute velocity; and therefore take a ſpace of time to perform one revolution in proportion to the radius; or, which is the ſame thing, the number of revolutions in the ſame given time, will be reciprocally as the length of the ſail.

Maxim 7. *The load at a maximum that ſails of a ſimilar figure and poſition will overcome, at a given diſtance from the center of motion, will be as the cube of the radius.*

Geometry informs us, that in ſimilar figures the ſurfaces are as the ſquares of their ſimilar ſides; of conſequence the quantity of cloth will be as the ſquare of the radius: alſo in ſimilar figures and poſitions, the impulſe of the wind, upon every ſimilar ſection of the cloth, will be in proportion to the ſurface of that ſection; and conſequently, the impulſe of the wind upon the whole, will be as the ſurface of the whole: but as the diſtance of every ſimilar ſection, from the center of motion, will be as the radius; the diſtance of the center of power of the whole, from the center of motion, will be as the radius alſo; that is, the lever by which the power acts, will be as the radius: as therefore the impulſe of the wind, reſpecting the quantity of cloth, is as the ſquare of the radius, and the lever, by which it acts, as the radius ſimply; it follows, that the load which the ſails will overcome, at a given diſtance from the center, will be as the cube of the radius.

Maxim 8. *The effect of ſails of ſimilar figure and poſition, are as the ſquare of the radius.*

By maxim 6. it is proved, that the number of revolutions made in a given time, are as the radius inverſely. Under maxim 7. it appears, that the length of the lever, by which the power acts, is as the radius directly; therefore theſe equal and oppoſite ratios deſtroy one another: but as in ſimilar figures the quantity of cloth is as the ſquare of the radius, and the action of the wind is in proportion to the quantity of cloth, as alſo appears under maxim 7; it follows that the effect is as the ſquare of the radius.

Corol. 1. Hence it follows, that augmenting the length of the ſail, without augmenting the quantity of cloth, does not increaſe the power; but becauſe what is gained by the length of the lever, is loſt by the ſlowneſs of the rotation.

Corol. 2. If ſails are increaſed in length, the breadth remaining the ſame, the effect will be as the radius.

IV. Con-

VI. *Concerning the Velocity of the Extremities of Wind-mill Sails, in Reſpect to the Velocity of the Wind.*

Maxim 9. *The velocity of the extremities of* Dutch *ſails, as well as of the enlarged ſails, in all their uſual poſitions when unloaded, or even loaded to a maximum, are conſiderably quicker than the velocity of the wind.*

The *Dutch* ſails unloaded, as in Tab. 3. N° 8. made 120 revolutions in 52″: the diameter of the ſails being 3 feet 6 inches, the velocity of their extremities will be 25,4 feet in a ſecond; but the velocity of the wind producing it, being 6 feet in the ſame time, we ſhall have 6 : 25,4 : : 1 : 4,2; in this caſe therefore, the velocity of their extremities was 4,2 times greater than that of the wind. In like manner, the relative velocity of the wind, to the extremities of the ſame ſails, when loaded to a *maximum*, making then 93 turns in 52″, will be found to be as 1 : 3,3; or 3,3 times quicker than that of the wind.

The following table contains 6 examples of *Dutch* ſails, and 4 examples of the enlarged ſails, in different poſitions, but with the conſtant velocity of the wind of 6 feet in a ſecond, from table 3: and alſo 6 examples of *Dutch* ſails in different poſitions, with different velocities of the wind from table 4.

TABLE V. *Containing the Ratio of the Velocity of the Extremities of Windmill Sails to the Velocity of the Wind.*

N°	N° of Tab. III. and IV.	Angle at the Extremity.	Velocity of the wind in a ſecond.	Ratio of the velocity of the wind and extremities of the ſails.		
				unloaded.	loaded.	
1	8	0°	6^{f} 0^{in}	1 : 4,2	1 : 3,3	From Table III.
2	9	3	6 0	1 : 4,2	1 : 2,8	
3	10	5	6 0	— —	1 : 2,75	
4	11	7	6 0	1 : 4,	1 : 2,7	
5	12	10	6 0	1 : 3,8	1 : 2,6	
6	13	12	6 0	1 : 3,5	1 : 2,3	
7	14	7½	6 0	1 : 4,3	1 : 2,6	
8	15	10	6 0	1 : 4,1	1 : 2,6	
9	16	12	6 0	1 : 4,	1 : 2,3	
10	17	15	6 0	1 : 3,35	1 : 2,2	
11	1	5	4 4½	1 : 4,	1 2,8	From Tab. IV.
12	2	5	8 9	1 : 4,3	1 2,6	
13	3	7½	4 4½	— —	1 : 2,8	
14	4	7½	8 9	— —	1 : 2,7	
15	5	10	4 4½	1 : 3,8	1 : 2,6	
16	6	10	8 9	1 : 3,4	1 : 23	
1	2	3	4	5	6	

It appears from the preceding collection of examples, that when the extremities of the *Dutch* ſails are parallel to the plane of motion, or at right angles to the wind, and to the axis, as they are made according to the common practice in *England*, that their velocity, unloaded, is above 4 times, and loaded to a *maximum*, above 3 times greater than that of the wind: but that when the Dutch ſails, or enlarged ſails, are in their beſt poſitions, their velocity unloaded is 4 times, and loaded to a *maximum*, at a medium the Dutch ſails are 2,7, and the enlarged ſails 2,6 times greater than the velocity of the wind. Hence we are furniſhed with a method of knowing the velocity of the wind, from obſerving the velocity of the windmill ſails; for knowing the radius, and the number of turns in a minute, we ſhall have the velocity of the extremities; which, divided by the following diviſors, will give the velocity of the wind.

Dutch ſails in the common poſition	unloaded	4.2
	loaded —	3.3
Dutch ſails in their beſt poſition -	unloaded	4.0
	loaded —	2.7
Enlarged ſails in the beſt poſition	unloaded	4.0
	loaded —	2.6

From the above diviſors there ariſes the following compendiums; ſuppoſing the radius to be 30 feet, which is the moſt uſual length in this country, and the mill to be loaded to a *maximum*, as is uſually the caſe with corn mills; for every 3 *turns in a minute, of the Dutch ſails in their common poſition, the wind will move at the rate of 2 miles an hour*; for every 5 *turns in a minute, of the Dutch ſails in their beſt poſition, the wind moves 4 miles an hour*; and for every 6 *turns in a minute, of the enlarged ſails in their beſt poſition, the wind will move 5 miles an hour.*

The following table, which was communicated to me by my friend Mr. Rouſe, and which appears to have been conſtructed with great care, from a conſiderable number of facts and experiments, and which having relation to the ſubject of this article; I here inſert it as he ſent it to me: but at the ſame time muſt obſerve, that the evidence for thoſe numbers where the velocity of the wind exceeds 50 miles an hour, do not ſeem of equal authority with thoſe of 50 miles an hour and under. It is alſo to be obſerved, that the numbers in col. 3. are calculated according to the ſquare of the velocity of the wind, which, in moderate velocities, from what has been before obſerved, will hold very nearly.

TABLE VI. *containing the Velocity and Force of Wind, according to their common Appellations.*

Velocity of the Wind.		Perpendicular force on one foot area in pounds averdupois.	Common appellations of the force of winds.
Miles in one Hour.	Feet in one ſecond.		
1	1,47	,005	Hardly perceptible.
2	2,93	,020	Juſt perceptible.
3	4,40	,044	
4	5,87	,079	Gentle pleaſant wind.
5	7,33	,123	
10	14,67	,492	Pleaſant briſk gale.
15	22,00	1,107	
20	29,34	1,968	Very briſk
25	36,67	3,075	
30	44,01	4,429	High winds.
35	51,34	6,027	
40	58,68	7,873	Very high.
45	66,01	9,963	
50	73,35	12,300	A ſtorm or tempeſt.
60	88,02	17,715	A great ſtorm.
80	117,36	31,490	An hurricane.
100	146,70	49,200	An hurricane that tears up trees, carries buildings before it, &c.
1	2	3	

VII. *concerning the abſolute Effect, produced by a given Velocity of the Wind, upon Sails of a given Magnitude and Construction.*

It has been obſerved by practitioners, that in mills with Dutch ſails in the common poſition, that when they make about 13 turns in a minute, they then work at a mean rate: that is, by the compendiums in the laſt article, when the velocity of the wind is 8⅔ miles an hour, or 12⅔ feet in a ſecond; which, in common phraſe, would be called a *freſh gale.*

The experiments ſet down in Tab. IV. Nº 4. were tried with a wind, whoſe velocity was 8¾ feet in a ſecond; conſequently had thoſe experiments been tried with a wind, whoſe velocity was 12⅔ feet in a ſecond, the effect, by maxim 3d, would have been 3 times greater; becauſe the cube of 12⅔ is 3 times greater than that of 8¾.

From Tab. 4. Nº 4. we find that the ſails, when the velocity of the wind was 8¾ feet in a ſecond, made 130 revolutions in a minute, with a load of 17,52 lb. From the meaſures of the machine, preceding the ſpecimen of a ſet of experiments, we find, that 20 revolutions of the ſails raiſed the ſcale and weight 11,3 inches: 130 revolutions will therefore raiſe the ſcale 73,45 inches, which, multiplied by 17,52 lb. makes a product of 1287, for the effect of the Dutch ſails in their beſt poſition; that is, when the velocity of the wind is 8¾ feet in a ſecond: this product therefore multiplied by three, will give 3861 for the effect of the ſame ſails, when the velocity of the wind is 12⅔ feet in a ſecond.

Deſaguliers makes the utmoſt power of a man, when working ſo as to be able to hold it for ſome hours, to be equal to that of raiſing an hogſhead of water 10 feet high in a minute. Now, an hogſhead conſiſting of 63 ale gallons, being reduced into pounds averdupois, and the height into inches; the product made by multiplying thoſe two numbers will be 76800; which is 19 times greater than the product of the ſails laſt-mentioned, at 12½ feet in a ſecond: therefore, by maxim 8th, if we multiply the ſquare root of 19, that is 4,46, by 21 inches, the length of the ſail producing the effect 3861, we ſhall have 93,66 inches, or 7 feet 9⅔ inches for the radius of a Dutch ſail in its beſt poſition, whoſe mean power ſhall be equal to that of a man: but if they are in their common poſition, their length muſt be increaſed in the ratio of the ſquare root of 442 to that of 639, as thus appears;

The ratio of the *maximum* products of N°. 8 and 11. Tab. III. are as 442 : 639: but by maxim 8, the effects of ſails of different radii are as the ſquare of the radii; conſequently the ſquare roots of the products or effects, are as the radii ſimply; and therefore as the ſquare root of 442 is to that of 639; ſo is 93,66 to 112,66; or 9 feet 4⅔ inches.

If the ſails are of the enlarged kind, then from Tab. III. N° 11 and 15. we ſhall have the ſquare root of 820 to that of 639 : : 93,66 : 82,8 inches, or 6 feet 10¾ inches: ſo that in round numbers we ſhall have the radius of a ſail, of ſimilar figure to their reſpective models, whoſe mean power ſhall be equal to that of a man;

The Dutch ſails in their common poſition	9½ feet.
The Dutch ſails in their beſt poſition ——	8
The enlarged ſails in their beſt poſition —	7

Suppoſe now the radius of a ſail to be 30 feet, and to be conſtructed upon the model of the enlarged ſails, N° 14 or 15. Tab. III. dividing 30 by 7 we, ſhall have 4,28, the ſquare of which is 18,3; and this, according to maxim 7, will be the relative power of a ſail of 30 feet, to one of 7 feet; that is, when working at a mean rate, the 30 feet ſail will be equal to the power of 18,3 men, or of $3\frac{2}{3}$ horſes; reckoning 5 men to a horſe: whereas the effect of the common Dutch ſails, of the ſame length, being leſs in the proportion of 820 : 442, will be ſcarce equal to the power of 10 men, or of 2 horſes.

That theſe computations are not merely ſpeculative, but will nearly hold good when applied to works in large, I have had an opportunity of verifying: for in a mill with the enlarged ſails of 30 feet, applied to the cruſhing of rape ſeed, by means of two runners upon the edge, for making oil; I obſerved, that when the ſails made 11 turns in a minute, in which caſe the velocity of the wind was about 13 feet in a ſecond, according to article 6th, that the runners then made 7 turns in a minute: whereas 2 horſes, applied to the ſame 2 runners, ſcarcely worked them at the rate of $3\frac{1}{2}$ turns in the ſame time. Laſtly, with regard to the real ſuperiority of the enlarged ſails, above the Dutch ſails as commonly made, it has ſufficiently appeared, not only in thoſe caſes where they have been applied to new mills, but where they have been ſubſtituted in the place of the others.

VIII. *Concerning horizontal Windmills and Water-Wheels, with oblique Vanes.*

Obſervations upon the effects of common windmills, with oblique vanes, have led many to imagine, that could the vanes be brought to receive the direct impulſe, like a ſhip ſailing before the wind, it would be a very great improvement in point of power: while others attending to the extraordinary and even unexpected effects of oblique vanes have been led to imagine that oblique vanes applied to water-mills, would as much exceed the common water wheels, as the vertical wind-mills are found to have exceeded all attempts towards an horizontal one. Both theſe notions, but eſpecially the firſt, have ſo plauſible an appearance, that of late years there has ſeldom been wanting thoſe, who have aſſiduouſly employed themſelves to bring to bear deſigns of this kind: it may not therefore be unacceptable to endeavour to ſet this matter in a clear light.

PLATE III. fig. 2. Let A B be the ſection of a plane, upon which let the wind blow in the direction C D, with ſuch a velocity as to deſcribe a given ſpace B E, in a given time (ſuppoſe 1 ſecond); and let A B be moved parallel to itſelf, in the direction C D. Now, if the plane A B moves with the ſame velocity as the wind; that is, if the point B moves through the ſpace B E in the ſame time that a particle of air would move through the ſame ſpace; it is plain that, in this caſe, there can be no preſſure or impulſe of the wind upon the plane: but if the plane moves ſlower than the wind, in the ſame direction, ſo that the point B may move to F, while a particle of air, ſetting out from B at the ſame inſtant, would move to E, then B F will expreſs the velocity of the plane; and the relative velocity of the wind and plane will be expreſſed by the line F. E. Let the ratio of F E to B E be given, (ſuppoſe 2 : 3); let the line A B repreſent the impulſe of the wind upon the plane A B, when acting with its whole velocity B E; but, when acting with its relative velocity F E, let its impulſe be denoted by ſome aliquot part of A B, as for inſtance $\frac{4}{9}$ A B: then will $\frac{4}{9}$ of the parallelogram A F repreſent the mechanical power of the plane; that is, $\frac{4}{9} AB \times \frac{1}{3} BE$.

2dly, Let I N be the ſection of a plane, inclined in ſuch a manner, that the baſe I K of the rectangle triangle I K N may be equal to A B; and the perpendicular N K = B E; let the plane I N be ſtruck by the wind, in the direction L M, perpendicular to I K: then, according to the known rules of oblique forces, the impulſe of the wind upon the plain I N, tending to move it according to the direction L M, or N K, will be denoted by the baſe I K; and that part of the impulſe, tending to move it according to the direction I K, will be expreſſed by the perpendicular N K. Let the plane I N be moveable in the direction of I K only; that is, the point I in the direction of I K, and the point N in the direction N Q, parallel thereto. Now it is evident, that if the point I moves through the line I K, while a particle of air, ſetting forwards at the ſame time from the point N, moves through the line N K, they will both arrive at the point K at the ſame time; and conſequently, in this caſe alſo, there can be no preſſure or impulſe of the particle of the air upon the plane I N. Now let I O be to I K as B F to B E; and let the plane I N move at ſuch a rate, that the point I may arrive at O, and acquire the poſition I Q, in the ſame time that a particle of wind would move through the ſpace N K: as O Q is parallel to I N; (by the properties of ſimilar triangles) it will cut N K in the point P, in ſuch a manner, that N P=B F, and P K=F E: hence it appears, that the plane I N, by acquiring the poſition O Q, withdraws itſelf from the action of the wind, by the ſame ſpace N P, that the plane A B does by acquiring the poſition F G; and conſequently, from the equality of P K to F E, the relative impulſe of the Wind P K, upon the plane O Q, will be equal to the relative impulſe of the wind F E, upon the plane F G: and ſince the impulſe of the wind upon A B, with the relative velocity F E, in the direction B E, is repreſented by $\frac{4}{9}$ A B; the relative impuſe of the wind upon the plane I N, in the direction N K, will in like manner be repreſented by $\frac{4}{9}$ I K; and the impulſe of the wind upon the plane I N, with the relative velocity P K, in the direction I K, will be repreſented by $\frac{4}{9}$ N K: and conſequently the mechanical power of the plane I N, in the direction I K, will be $\frac{4}{9}$ the parallellogram I Q: that is $\frac{1}{3}$ I K × $\frac{4}{9}$ N K: that is, from the equality of I K=A B and N K=B E, we ſhall have $\frac{4}{9}$ I Q=$\frac{1}{3}$ A B × $\frac{4}{9}$ B E=$\frac{4}{9}$ A B × $\frac{1}{3}$ B E=$\frac{}{9}$ of the area of the parallellogram A F. Hence we deduce this

GENERAL PROPOSITION,

That all planes, however ſituated, that intercept the ſame ſection of the wind, and having the ſame relative velocity, in regard to the wind, when reduced into the ſame direction, have equal powers to produce mechanical effects.

For what is loſt by the obliquity of the impulſe, is gained by the velocity of the motion.

Hence it appears, that an oblique ſail is under no diſadvantage in reſpect of power, compared with a direct one; except what ariſes from a diminution of its breadth, in reſpect to the ſection of the wind: the breadth I N being by obliquity reduced to I K.

The diſadvantage of horizontal windmills therefore does not conſiſt in this; that each ſail, when directly expoſed to the wind, is capable of a leſs power, than an oblique one of the ſame dimenſions; but that in an horizontal windmill, little more than one ſail can be acting at once: whereas in the common windmill, all the four act together: and therefore, ſuppoſing each vane of an horizontal windmill, of the ſame dimenſions as each vane of the vertical, it is manifeſt the power of a vertical mill with four ſails, will be four times greater than the power of the horizontal one, let its number of vanes be what it will: this diſadvantage ariſes from the nature of the thing; but if we conſider the further diſadvantage, that ariſes from the difficulty of getting the ſails back again againſt the wind, &c. we need not wonder if this kind of mill is in reality found to have not above $\frac{1}{8}$ or $\frac{1}{10}$ of the power of the common ſort; as has appeared in ſome attempts of this kind.

In like manner, as little improvement is to be expected from water-mills with oblique vanes: for the power of the ſame ſection of a ſtream of water, is not greater when acting upon an oblique vane, than when acting upon a direct one: and any advantage that can be made by intercepting a greater ſection, which ſometimes may be done in the caſe of an open river, will be counterbalanced by the ſuperior reſiſtance, that ſuch vanes would meet with by moving at right angles to the current: whereas the common floats always move with the water nearly in the ſame direction.

Here it may reaſonably be aſked, that ſince our geometrical demonſtration is general, and proves, that one angle of obliquity is as good as another; why in our experiments it appears, that there is a certain angle which is to be preferred to all the reſt? It is to be obſerved, that if the breadth of the ſail I N is given, the greater the angle K I N, and the leſs will be the baſe I K: that is, the ſection of wind interſected, will be leſs: on the other hand, the more acute the angle K I N, the leſs will be the perpendicular K N: that is, the impulſe of the wind, in the direction I K being leſs, and the velocity of the ſail greater; the reſiſtance of the medium will be greater alſo. Hence therefore, as there is a diminution of the ſection of the wind intercepted on one hand, and an increaſe of reſiſtance on the other, there is ſome angle, where the diſadvantage ariſing from theſe cauſes upon the whole is the leaſt of all; but as the diſadvantage ariſing from reſiſtance is more of a phyſical than geometrical conſideration, the true angle will beſt be aſſigned by experiments.

SCHOLIUM.

In trying the experiments contained in Tab. III. and IV. the different ſpecific gravity of the air, which is undoubtedly different at different times, will cauſe a difference in the load, proportional to the difference of its ſpecific gravity, though its velocity remains the ſame; and a variation of ſpecific gravity may ariſe not only from a variation of the weight of the whole column, but alſo by the difference of heat of the air concerned in the experiment, and poſſibly of other cauſes; yet the irregularities that might ariſe from a difference of ſpecific gravity were thought to be too ſmall to be perceivable, till after the principal experiments were made, and their effects compared; from which, as well as ſucceeding experiments, thoſe variations were found to be capable of producing a ſenſible, though no very conſiderable effect: however, as all the experiments were tried in the ſummer ſeaſon, in the day-time, and under cover; we may ſuppoſe that the principal ſource of error would ariſe from the different weight of the column of the atmoſphere at different times: but as this ſeldom varies above $\frac{1}{1}$, part of the whole, we may conclude, that though many of the irregularities contained in the experiments referred to in the foregoing eſſay, might ariſe from this cauſe; yet as all the principal concluſions are drawn from the medium of a conſiderable number, many whereof were made at different times, it is preſumed that they will nearly agree with the truth, and be altogether ſufficient for regulating the practical conſtruction of thoſe kind of machines, for which uſe they were principally intended.

AN
EXPERIMENTAL EXAMINATION
OF THE QUANTITY AND PROPORTION
OF MECHANIC POWER
NECESSARY TO BE EMPLOYED IN GIVING DIFFERENT DEGREES OF VELOCITY TO
HEAVY BODIES
FROM A
STATE OF REST.

By Mr. JOHN SMEATON, F. R. S.

A N

EXPERIMENTAL EXAMINATION, &c.

Read before the Royal Society, April, 25, 1776.

ABOUT the year 1686 Sir ISAAC NEWTON firſt publiſhed his *Principia*, and, conformably to the language of mathematicians of thoſe times defined, that “the quantity of motion “is the meaſure of the ſame, ariſing from the velocity and “quantity of matter conjointly.” Very ſoon after this publication, the truth or propriety of this definition was diſputed by certain philoſophers, who contended, that the meaſure of the quantity of motion ſhould be eſtimated by taking the quantity of matter and the ſquare of the velocity conjointly. There is nothing more certain, than that from equal impelling powers, acting for equal intervals of time, equal increaſes of velocity are acquired by given bodies, when unreſiſted by a medium; thus gravity cauſes a body, in obeying its impulſe during one ſecond of time, to acquire a velocity which would carry it uniformly forward, without any additional impulſe, at the rate of 32 ft. 2 in. *per* ſecond; and if gravity is ſuffered to act upon it for two ſeconds, it will have, in that time, acquired a velocity, that would carry it, at an uniform rate, juſt double of the former; that is, at the rate of 64ft. 4 in. *per* ſecond. Now, if in conſequence of this equal increaſe of velocity, in an equal increaſe of time, by the continuance of the ſame impelling power, we define that to be a double quantity of motion, which is generated in a given quantity of matter, by the action of the ſame impelling power for a double time; this will be co-incident with Sir ISAAC NEWTON's definition above mentioned; whereas, in trying experiments upon the total effects of bodies in motion, it appears, that when a body is put in motion, by whatever cauſe, the impreſſion it will make upon an uniformly reſiſting medium, or upon uniformly yielding ſubſtances, will be as the maſs of matter of the moving body, multiplied by the ſquare of its velocity: the queſtion, therefore, properly is, whether thoſe terms, the *quantity of motion*, the *momenta* of bodies in motion, or *forces* of bodies in motion, which have generally been eſteemed ſynonymous, are with the moſt propriety of language to be eſteemed equal, double, or triple, when they have been generated by an equable impulſe, acting for an equal, double, or triple time; or that it ſhould be meaſured by the effects being equal, double, or triple, in overcoming reſiſtances before a body in motion can be ſtopped? For, according as thoſe terms are underſtood in this or that way, it will neceſſarily follow, that the *momenta* of equal bodies will be as the velocities, or as the ſquares of the velocities, or as the ſquares of the velocities reſpectively; it being certain, that, whichever we take for the proper definition of the term quantity of motion, by paying a proper regard to the collateral circumſtances that attend the application of it, the ſame concluſion, in point of computation, will reſult. I ſhould not, therefore, have thought it worth while to trouble the Society upon this ſubject, had I not found, that not only myſelf and other practical artiſts, but alſo ſome of the moſt approved writers, had been liable to fall into errors, in applying theſe doctrines to practical mechanics, by ſometimes forgetting or neglecting the due regard which ought to be had to theſe collateral circumſtances. Some of theſe errors are not only very conſiderable in themſelves, but alſo of great conſequence to the public, as they tend greatly to miſlead the practical artiſt in works that occur daily, and which often require very great ſums of money in their execution. I ſhall mention the following inſtances.

DESAGULIERS, in his ſecond volume of Experimental Philoſophy, treating upon the queſtion concerning the forces of bodies in motion, after taking much pains to ſhew that the diſpute, which had then ſubſiſted fifty years, was a diſpute about the meaning of words; and that the ſame concluſion will be brought out, when things are rightly underſtood, either upon the old or new opinion, as he diſtinguiſhes them; among other things, tells us, that the old and new opinion may be eaſily reconciled in this inſtance: that the wheel of an underſhot water-mill is capable of doing quadruple work when the velocity of the water is doubled, inſtead of double work only; "because (the adjutage being the ſame, ſays he, we find, that "as the water's velocity is double, there are twice the number "of particles of water that iſſue out, and therefore the ladle-"board is ſtruck by twice the matter, which matter moving "with twice the velocity that it had in the firſt caſe, the whole "effect muſt be quadruple, though the inſtantaneous ſtroke of "each particle is increaſed only in a ſimple proportion of the "velocity." See vol. II. Annotations on lecture 6th, p. 92.

Again, in the ſame volume, lecture 12th, p. 424. referring to what went before, he tells us, "The knowledge of the fore-"going particulars is abſolutely neceſſary for ſetting an under-"ſhot wheel to work; but the advantage to be reaped from it "would be ſtill gueſswork, and we ſhould be ſtill at a loſs to "find out the utmoſt it can perform, if we had not an in-"genious propoſition of that excellent mechanic M. PARENT,

“ of the Royal Academy of Sciences, who has given us a “ *maximum* in this caſe, by ſhewing, that an underſhot wheel “ can do the moſt work, when its velocity is equal to the “ third part of the velocity of the water that drives it, &c. “ becauſe then two-thirds of the water is employed in driving “ the wheel with a force proportionable to the ſquare of its “ velocity. If we multiply the ſurface of the adjutage or open- “ ing by the height of the water, we ſhall have the column of “ water that moves the wheel. The wheel thus moved will “ ſuſtain on the oppoſite ſide only four-ninths of that weight, “ which will keep it in equilibrio; but what it can move with “ the velocity it goes with, will be but one-third of that weight “ of equilibrium; that is, $\frac{4}{27}$ths of the weight of the firſt “ column, &c.—This is the utmoſt that can be expected.”

The ſame concluſion is likewiſe adopted by MACLAURIN, in art. 907. p. 728. of his Fluxions, where, giving the fluxionary deduction of M. PARENT's propoſition, he ſays, “ that if A re- “ preſents the weight which would balance the force of the “ ſtream, when its velocity is *a*; and U repreſents the velocity “ of the part of the engine, which it ſtrikes when the motion “ of the machine is uniform, &c.—the machine will have the “ greateſt effect when U is equal to $\frac{a}{3}$; that is, if the weight “ that is raiſed by the engine be leſs than the weight which “ would balance the power, in the proportion of 4 to 9, and the “ *momentum* of the weight is $\frac{4Aa}{27}$.”

Finding that theſe concluſions were far from the truth; and ſeeing, from many other circumſtances, that the practical theory of making water and wind-mills was but very imperfectly delivered by any author I had then an opportunity of conſulting *;
in

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* BELIDOR, *Architecture Hydraulique*, greatly prefers the application of water to an underſhot mill, inſtead of an overſhot; and
attempts

in the year 1751 I began a courſe of experiments upon this ſubject. Theſe experiments, with the concluſions drawn from them, have already been communicated to this Society, who printed them in vol. LI. of their Tranſactions for the year 1759, and for this communication I had the honour of receiving the annual medal of Sir GODFREY COPLEY, from the hands of our very worthy Preſident the late Earl of MACCLESFIELD. Thoſe experiments and concluſions ſtand uncontroverted, ſo far as I know, to this day; and having ſince that time been concerned in directing the conſtruction of a great number of mills, which were all executed upon the principles deduced from them, I have by that means had many opportunities of comparing the effects actually produced with the effects which might be expected from the calculation; and the agreement, I have always found between theſe two, appears to me fully to eſtabliſh the truth of the

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attempts to demonſtrate, that water applied underſhot will do ſix times more execution than the ſame applied overſhot. See vol. I. p. 286. While DESAGULIERS, endeavouring to invalidate what had been advanced by BELIDOR, and greatly preferring an overſhot to an underſhot, ſays, Annotat. on lecture 12. vol. II. p. 532. that from his own experience, "a well-made overſhot mill ground as much "corn in the ſame time with ten times leſs water;" ſo that betwixt BELIDOR and DESAGULIERS, here is a difference of no leſs than 60 to 1.

Again, BELIDOR, vol. II. p. 72. ſays, that the centre of gravity of each ſail of a wind-mill ſhould travel in its own circle with one-third of the velocity of the wind; ſo that, taking the diſtance of this centre of gravity from the centre of motion at 20 feet, as he ſtates it p. 38. art. 849. the circumference will be exceeding 126 feet Engliſh meaſure: a wind, therefore, to make the mill go twenty turns *per* minute, which they frequently do with a freſh wind and all their cloth ſpread, would require the wind to move above eighty miles an hour; a velocity perhaps hardly equalled in the greateſt ſtorms we experience in this climate.

principles upon which they were conſtructed, when applied to great works, as well as upon a ſmaller ſcale in models.

Reſpecting the explanatory deduction of **DESAGULIERS** in the firſt example abovementioned, which, indeed, I have found to be the commonly received doctrine among theoretical mechanics, it is ſhewn, in my former Eſſay, page 21, 22, and 24, part 1. maxim 4. that, where the velocity of water is double, the adjutage or aperture of the ſluice remaining the ſame, the effect is eight times; that is, not as the ſquare but as the cube of the velocity; and the ſame is inveſtigated concerning the power of the wind ariſing from difference of velocity, p. 52. being part 3. maxim 4.

The concluſion in the ſecond example abovementioned, adopted both by **DESAGULIERS** and **MACLAURIN**, is not leſs wide of the truth than the foregoing; for if that concluſion were true, only $\frac{4}{27}$ths of the water expended could be raiſed back again to the height of the reſervoir from which it had deſcended, excluſively of all kinds of friction, &c, which would make the actual quantity raiſed back again ſtill leſs; that is, leſs than one-ſeventh of the whole; whereas it appears, from table 1. of the preceding eſſay, that in ſome of the experiments there related, even upon the ſmall ſcale on which they were tried, the work done was equivalent to the raiſing back again about one quarter of the water expended; and in large works the effect is ſtill greater, approaching towards half, which ſeems to be the limit for the underſhot mills, as the whole would be the limit for the overſhot mills, if it were poſſible to ſet aſide all friction, reſiſtance from the air, &c. ſee p. 29.

The velocity alſo of the wheel, which, according to M. **PARENT'S** determination, adopted by **DESAGULIERS** and **MACLAURIN**, ought to be no more than one-third of that of the water, varies at the *maximum* in the abovementioned experiments of table I. between one-third and one-half; but in all the caſes there related, in which the moſt work is performed in proportion to the water expended, and which approach the neareſt to the circumſtances of great works, when properly executed, the *maximum* lies much nearer to one-half than one-third; one-half ſeeming to be the true *maximum*, if nothing were loſt by the reſiſtance of the air, the ſcattering of the water carried up by the wheel, and thrown off by the centrifugal force, &c. all which tend to diminiſh the effect more, at what would be the *maximum* if theſe did not take place, than they do when the motion is a little ſlower.

Finding theſe matters, as well as others, to come out in the experiments, ſo very different from the opinions and calculations of authors of the firſt reputation, who, reaſoning according to the Newtonian definition, muſt have been led into theſe errors from a want of attending to the proper collateral circumſtances; I thought it very material, eſpecially for the practical artiſt, that he ſhould make uſe of a kind of reaſoning in which he ſhould not be ſo liable to miſtakes; in order, therefore, to make this matter perfectly clear to myſelf, and poſſibly ſo to others, I reſolved to try a ſet of experiments from whence it might be inferred, what proportion or quantity of mechanical power is expended in giving the ſame body different degrees of velocity. This ſcheme was put in execution in the year 1759, and the experiments were then ſhewn to ſeveral friends, particularly my very worthy and ingenious friend Mr. WILLIAM RUSSELL.

In my experimental inquiry concerning the powers of water and wind before referred to, I have, p. 105. part I. defined what I meant by power, as applied to practical mechanics, that is, what I now call mechanical power; which, in terms ſynonymous to thoſe there uſed, may be ſaid to be meaſured by multiplying the weight of the body into the perpendicular height from which it can deſcend; thus the ſame weight, deſcending from a double height, is capable of producing a double mechanical effect, and is therefore a double mechanical power. A double weight deſcending from the ſame height is alſo a double power, becauſe it likewiſe is capable of producing a double effect; and a given body, deſcending through a given perpendicular height, is the ſame power as a double body deſcending through half that perpendicular; for, by the intervention of proper levers, they will counter-balance one another, conformably to the known laws of mechanics, which have never been controverted. It muſt, however, be always underſtood, that the deſcending body, when acting as a meaſure of power, is ſuppoſed to deſcend ſlowly, like the weight of a clock or a jack; for, if quickly deſcending, it is ſenſibly compounded with another law, *viz.* the law of acceleration by gravity.

DESCRIPTION OF THE MACHINE.

A B is the baſe of the machine placed upon a table.

A C is a pillar or ſtandard.

C D is an arm, upon the extremity of which is fixed a plate *f g*, which is here ſeen edge-ways, through which is a ſmall hole for receiving a ſmall ſteel pivot *e*, fixed in the top of the upright axis *e* B; the lower end of this axis finiſhes in a conical ſteel point, which reſts upon a ſmall cup of hard ſteel poliſhed at B.

H I is a cylinder of white fir, which paſſes through a perforation in the axis, and therein fixes; and, upon the two arms formed thereby, are capable of ſliding.

K L two cylindric weights of lead of equal ſize, which are capable of being fixed upon any part of the cylindric arms, from the axis to their extremities, by means of two thin wedges of wood. The two weights, therefore, being at equal diſtances from the centre, and the axis perpendicular, the whole will be balanced upon the point at B, and moveable thereupon by an impelling power, with very little friction.

Upon the upper part of the axis are formed M N, two cylindrical barrels, whereof M is double the diameter of N; they have a little pin ſtuck into one ſide of each at *o*, *p*.

Q is a piece capable of ſliding higher or lower, as occaſion requires; and carries

R, a light pulley of about three inches diameter, hung upon a ſteel axis, and moveable upon two ſmall pivots. The plane of the pulley, however, is not directed to the middle of the upright axis, but a little on one ſide, ſo as to point (at a mean) between the ſurface of the bigger barrel and the leſs.

S is a light ſcale for receiving weights, and hangs by a ſmall twine, cord, or line, that paſſes the pulley, and terminates either upon the bigger barrel or the leſs, as may be required; the ſliding-piece Q being moved higher or lower for each, that the line, in paſſing from the pulley to the barrel, may be nearly horizontal. The end of the line, that is furtheſt from the ſcale, is terminated by a ſmall loop, which hangs on upon the pin *o*, or the pin *p*, according as the bigger or the leſſer barrel is to be uſed.

Now, having wound up a certain number of turns of the line upon the barrel, and having placed a weight in the ſcale S, it is obvious, that it will cauſe the axis to turn round, and give motion to its arms, and to the weights of lead placed thereon, which are the heavy bodies to be put in motion by the impulſe of the weight in the ſcale; and when the line is wound off to the pin, the loop ſlips off; and the ſcale then falling down, the weight will ceaſe to accelerate the motion of the heavy bodies, and leave them revolving, equably forward, with the velocity they have acquired, except ſo far as it muſt be gradually leſſened by the friction of the machine and reſiſtance of the air, which being ſmall, the bodies will revolve ſometime before their velocity is apparently diminiſhed.

MEASURES OF SOME PARTS OF THE MACHINE.

	Inches.
Diameter of the cylinders of lead, or the heavy bodies	2,57
Length of ditto, - - - -	1,56
Diameter of the hole therein, - - -	,72

Weight of each cylinder 3 lbs. Averdupois.

Greater diſtance of the middle of each body from the centre of the axis, - - -	8,25
The ſmaller diſtance of ditto, - -	3,92
10 turns of the ſmaller barrel raiſes the ſcale, 5 ditto of the bigger ditto, - - -	25,25

When the bodies are at the ſmaller diſtance above ſpecified from the axis of rotation, they are then in effect at half the greater diſtance from that axis; for, ſince the axis itſelf, and the cylindric arms of wood, keep an unvaried diſtance from the centre of rotation, the bodies themſelves muſt be moved nearer than half their former diſtance, in order that, compounded with the unvariable parts, they may be virtually at the half diſtance. In order to find this half diſtance nearly, I put in an arm of the ſame wood, that only went through the axis, without extending in the oppoſite direction; one of the bodies being put upon the end of this arm, at the diſtance of 8,25 inches, the whole machine was inclined till the body and arm became a kind of pendulum,

dulum, and vibrated at the rate of 92 times *per* minute; and as a pendulum of the half length vibrates quicker in the proportion of $\sqrt{1}$ to $\sqrt{2}$; that is, in the proportion of 92 to 130 nearly; therefore, keeping the ſame inclination of the machine, the weight was moved upon the arm till it made 130 vibrations *per* minute; which was found to be, when it was at 3,92 inches diſtance from the centre as above ſtated, which is about $\frac{2}{16}$ths of an inch nearer than the half diſtance. The double arm was then put in, and marked accordingly, and the bodies being mounted thereon, the whole was adjuſted ready for uſe; and with it were tried the following experiments, each of which was repeated ſo many times as to be fully ſatisfactory.

TABLE OF EXPERIMENTS.

N°.	Ounces Avoirdupois in the Scale.	Barrel uſed, M the bigger, N the ſmaller.	The Arms, W the whole, H the half-length.	Number of Turns of the Line wound on the Barrel.	Time of the Deſcent of the Weight in the Scale.	Time in making 20 Revolutions with equal motions.
					"	'
1	8	M	W	5	14¼	29
2	8	N	W	10	28¼	29¼
3	8	N	W	2½	14¼	58½
4	32	M	W	5	7	14
5	32	N	W	10	14	14½
6	32	N	W	2½	7	28¼
7	8	M	H	5	7	14¼
8	8	N	H	10	14	15
9	8	N	H	2½	7	30¼
1	2	3	4	5	6	7

The 58″½ in number 3, column 7, was determined in fact from 29″¼, being the time of making 10 equable revolutions after the weight was dropped off, in order to prevent the ſenſible retardation that might take place, and affect the obſervation, if continued for 20 revolutions made ſo ſlowly.

FURTHER DEFINITIONS.

I have already defined what I mean by mechanic power; but, before I proceed further, it will be neceſſary alſo to define the following terms:

Impulſe or Impulſion,
Impulſive Force or Power,
Impelling Force or power,

By all which, I underſtand the uniform endeavour that one body exerts upon another, in order to make it move; and that, whether it produces or generates motion by this endeavour or not; and the quantity of this impelling power may be meaſured either by its being a weight of itſelf, or by being counter-balanced by a weight. It may alſo act either immediately upon the body to be moved, ſo that if motion is the conſequence, they move with the ſame velocity; and that, either by a ſimple contact, or by being drawn as by a cord, or puſhed as by a ſtaff: or it may act by the intervention of a lever or other mechanic inſtrument, in which the velocity of the body to be moved may be very different from the velocity of the impelling power or mover; but in comparing them, the impelling powers muſt be reduced according to the proportional velocities of the mover and moved; or, in levers of different lengths, they may be compared by a ſtandard length of lever, which is the method taken in the ſubſequent reaſoning upon the preceding experiments. An impelling power, therefore, conſiſting of a double weight, or requiring a double weight to counter-balance it, when acting with equal levers, is a double impelling power, or an impelling power of double the intenſity.

OBSERVATIONS AND DEDUCTIONS FROM THE PRECEDING EXPERIMENTS.

1ſt, By the firſt experiment it appears, that the mechanic power employed, conſiſting of 8 ounces in the ſcale, deliberately deſcending (by 5 turns of the bigger barrel) through a perpendicular ſpace 25¼ inches, will repreſent the quantity of mechanic power which cauſes the two heavy bodies, from a ſtate of reſt, to acquire a velocity, ſuch as to carry them equably through 20 circumferences of their circle of revolution in the ſpace of 29″; and that the time in which the mechanic power produced this effect was 14″¼, as appears by column 6th. And this mechanic power we ſhall expreſs by the number 202, the product of the number of ounces in the ſcale multiplied by the inches in its perpendicular deſcent, for $8\times25\frac{1}{4}=202$.

2d, By the ſecond experiment, as 10 turns of the ſmaller barrel are equal to the ſame perpendicular height as 5 turns of the bigger, it follows, that the ſame mechanic power, *viz.* 202, acting upon the ſame heavy bodies to accelerate them, produces the very ſame effect in generating motion in the bodies as it did before, *viz.* 20 revolutions in 29″¼, the ſmall difference of ¼ of a ſecond being no more than may reaſonably be attributed to the unavoidable errors ariſing from friction of the machine, want of perfect accuracy in its meaſures, reſiſtance of the air, and imperfections in the obſervations themſelves, which muſt not only be allowed for in this, but the reſt; but as the impelling power is acting here upon a lever of but half the length, and, conſequently, but half the intenſity, when referred to the bodies to be moved, it takes juſt double the time to generate the ſame velocity therein.

DEDUCTION. It appears from hence, that the ſame mechanic power is capable of producing the ſame velocity in a given body, whether it is applied ſo as to produce it in a greater or leſſer time; but that the time taken to produce a given velocity, by an uniformly continued action, is in a ſimple inverſe proportion of the intenſity of the impulſive power.

3dly, The third experiment being made with 2½ turns of the leſſer barrel, the ſame weight in the ſcale of 8 ounces deſcending only 1-quarter part of the former perpendicular, the mechanic power employed will be only one quarter part of the former, *viz.* 50½; but as one quarter part of the mechanic power produces half of the former velocity in the heavy bodies; that is, they make 20 revolutions in 58″½; that is, nearly 10 revolutions in 29″; we may conclude, in this inſtance, that the mechanic power, employed in producing motion, is as the ſquare of the velocity produced in the ſame body; and that the velocity produced is as the time that an impelling power, of the ſame intenſity, continues to act upon it, as appears by the near agreement of numbers 2 and 3, column 6th.

4thly, In the fourth experiment, the apparatus is the ſame as the firſt, only here the weight in the ſcale is 32 ounces; that is, the impelling power is the quadruple of the firſt, and hereby a double velocity is given to the bodies; for they make 20 revolutions in 14″, which is a ſmall matter leſs than half the time taken up in making 20 revolutions in the firſt experiment. It alſo appears, that the velocity acquired is ſimply as the impelling power compounded with the time of its action; for a quadruple impulſion acting for 7″ inſtead of 14″ generates a double velocity, while the mechanic power employed to generate it is quadruple, for $32 \times 25\frac{1}{4} = 808$. And here the mechanic power employed being four times greater than the firſt, it holds here alſo, that the mechanic power, to be neceſſarily employed, is as the ſquare of the velocity to be generated; that is, in the ſame proportion as turned out in the third experiment, where the mechanic power employed was only a quarter part of the firſt.

5thly, The fifth and ſixth experiments were made with a mechanic power four times greater than thoſe employed in numbers 2 and 3 reſpectively; and ſince the ſame deductions reſult from hence as from numbers 2 and 3, they are additional confirmations of the concluſions drawn from them and from the laſt article.

6thly, In the ſeventh experiment, the diſpoſition of the apparatus is the ſame as number 1, only here the bodies are placed upon the arms at the half-length; from whence it appears, that the ſame mechanic power ſtill produces the ſame velocity in the ſame bodies; for though 20 revolutions were performed in 14¾" (ſee column 7) which is nearly half the time that 20 revolutions were performed in the firſt experiment; yet, ſince the circles in which the bodies revolved in the ſeventh are only of half the circumference as thoſe of number 1, it is obvious, that the abſolute velocity acquired by the moving bodies in the two caſes is equal. But, by column 6th, the time in which it was generated is only half; yet, notwithſtanding, this will coincide with the former concluſions, if the intenſity of the impelling power is compounded therewith; for, though the barrel was the ſame with the ſame number of turns as in number 1, and therefore the lever the ſame, by which the impelling power acted, yet, as the bodies, upon which this lever was to act, were placed upon a lever of only half the length from the centre, the impelling power acting by the firſt lever, would act upon the ſecond with double the intenſity, according to the known laws of mechanics; that is, it would require a double weight oppoſing the bodies, to prevent their moving, in order to balance it. An impulſive power, therefore, of double the intenſity, acting for half the time, produces the ſame effect in generating motion, as an impulſive power, of half the intenſity, acting for the whole time.

7thly, The eighth and ninth experiments afford the ſame deductions and confirmations relative to the ſeventh experiment, that the fifth and ſixth do reſpecting the fourth, and that the ſecond and third do reſpecting the firſt; and from the near agreement of the whole, when the neceſſary allowances before mentioned are made, together with ſome ſmall inequality ariſing from the mechanical power loſt by the difference of the motion given by gravity to the weight in the ſcale: I ſay, from theſe agreements, under the very different mechanical powers applied, which were varied in the proportion of 1 to 16, we may ſafely conclude, that this is the univerſal law of nature, reſpecting the capacities of bodies in motion to produce mechanical effects, and the quantity of mechanic power neceſſary to be employed to produce or generate different velocities (the bodies being ſuppoſed equal in their quantity of matter); that the mechanic powers to be expended are as the ſquares of the velocities to be generated, and *vice verſâ*; and that the ſimple velocities generated are as the impelling power compounded with, or multiplied by, the time of its action, and *vice verſâ*.

We ſhall, perhaps, form a ſtill clearer conception of the relation between velocities produced, and the quantity of mechanic power required to produce them; together with the collateral circumſtances attending, by which theſe propoſitions, ſeemingly two, are reconciled and united, by ſtating the following popular elucidation, which indeed was the original idea that occurred to me on conſidering this ſubject; to put which to an experimental proof gave birth to the foregoing apparatus and experiments.

Suppoſe then a large iron ball of 10 feet diameter, turned truly ſpherical, and ſet upon an extended plane of the ſame metal, and truly level. Now, if a man begins to puſh at it, he will find it very reſiſting to motion at firſt; but, by continuing the impulſe, he will gradually get it into motion, and having nothing to reſiſt it but the air, he will, by continuing his efforts, at length get it to roll almoſt as faſt as he can run.

Suppoſe now, in the firſt minute he gets it rolled through a ſpace of one yard; by this motion, proceeding from reſt (ſimilar to what happens to falling bodies) it would continue to roll forward at the rate of two yards *per* minute, without further help; but ſuppoſing him to continue his endeavours, at the end of another minute he will have given it a velocity capable of carrying it through a ſpace of two yards more, in addition to the former, that is, at the rate of four yards *per* minute; and at the end of the third minute, he has again added an equal increaſe of velocity, and made it proceed at the rate of ſix yards per minute, and ſo on, increaſing its velocity at the rate of two yards in every minute. The man, therefore, in the ſpace of every minute exerts an equal impulſe upon the ball, and generates an equal increaſe of movement correſpondent to the definition of Sir ISAAC NEWTON. But let us ſee what happens beſides: the man, in the firſt minute, has moved but one yard from where he ſet out; but he muſt in the ſecond minute move two yards more, in order to keep up with the ball; and as he exerted an impulſe upon it, ſo as at the end of the ſecond minute to have given it an additional velocity of the two yards, he muſt alſo in this time have gradually changed its velocity from the rate of two yards *per* minute to that of four, and the ſpace, that he will of conſequence have actually been obliged to go through in the ſecond minute, will be according to the mean of the extremes of velocity at the beginning and end thereof, that is, three yards in the ſecond minute; ſo that being one yard from his original place at the begining of the ſecond minute, at the end of it he will have moved the ſum of the journies of the firſt and ſecond minute, that is, in the whole four yards from his original place. As he has now generated a velocity in the ball of four yards *per* minute, in the third minute he muſt travel four yards to keep up with the ball, and one more in generating the equal increment of velocity; ſo that in the third minute, he muſt travel five yards to keep up the ſame impelling power upon the ball that he did in the firſt minute in travelling one, ſo that theſe five yards in the third minute added to the four yards that he had travelled in the two preceding minutes, ſets him at the end of the third minute nine yards from whence he ſet out, having then given the ball a velocity capable of carrying it uniformly forward at the rate of ſix yards *per* minute, as before ſtated. We may now leave the further purſuit of theſe proportions, and ſee how the account ſtands. He generated a velocity of two yards *per* minute in the firſt minute, the ſquare of which is four, when he had moved but one yard from his place; and he had generated a velocity of ſix yards *per* minute, the ſquare of which is thirty-ſix, at the end of the third minute, when he had travelled nine yards from his place. Now, ſince the ſquare of the velocity, generated at the end of the firſt minute, is to that of the velocity generated at the end of the third minute, as 4 : 36, that is, as 1 : 9; and ſince the ſpaces, moved through by the man to communicate theſe velocities, are alſo as 1 : 9, it follows, that the ſpaces through which the man muſt travel, in order to generate theſe velocities reſpectively (preſerving the impelling power perfectly equal), muſt be as the ſquares of the velocities that are communicated to the ball; for, if the man was to be brought back again to his original place by a mechanical power, equally exerted upon the man equally reſiſting, this would be the meaſure of what the man has done in order to give motion to the ball. It therefore directly follows, conformably to what has been deduced from the experiments, that the mechanic power that muſt of neceſſity be employed in giving different degrees of velocity to the ſame body, muſt be as the ſquare of that velocity; and if the converſe of this propoſition did not hold, *viz.* that if a body in motion, in being ſtopped, would not produce a mechanical effect equal or proportional to the ſquare of its velocity, or to the mechanical power employed in producing it, the effect would not correſpond with its producing cauſe.

Thus the conſequences of generating motion upon a level plane exactly correſpond with the generating of motion by gravity; *viz*, that though in two ſeconds of time the equal impulſive power of gravity gives twice the velocity to a body that it does in one ſecond, yet this collateral circumſtance attends it, that at the end of the double time, in conſequence of the velocity acquired in the firſt half, the body has fallen from where it ſet forward through four times the perpendicular; and therefore, though the velocity is only doubled, yet four times the mechanical power has been conſumed in producing it, as four times the mechanical power muſt be expended in bringing up the fallen body to its firſt place.

This then appears to be the foundation, not only of the diſputes that have ariſen, but of the miſtakes that have been made, in the application of the different definitions of quantity of motion; that while thoſe, that have adhered to the definition of Sir ISAAC NEWTON, have complained of their adveſaries, in not conſidering the time in which effects are produced, they themſelves have not always taken into the account the ſpace that the impelling power is obliged to travel through, in producing the different degrees of velocity. It ſeems, therefore, that, without taking in the collateral circumſtances both of time and ſpace, the terms, quantity of motion, *momentum*, and force of bodies in motion, are abſolutely indefinite; and that they cannot be ſo eaſily, diſtinctly, and fundamentally compared, as by having recourſe to the common meaſure, *viz.* mechanic power.

From the whole of what has been inveſtigated, it therefore appears, that time, properly ſpeaking, has nothing to do with the production of mechanical effects, otherwiſe than as, by equally flowing, it becomes a common meaſure; ſo that, whatever mechanical effect is found to be produced in a given time, the uniform continuance of the action of the ſame mechanical power will, in a double time, produce two ſuch effects, or twice that effect. A mechanical power, therefore, properly ſpeaking, is meaſured by the whole of its mechanical effect produced, whether that effect is produced in a greater or a leſſer time; thus, having treaſured up 1000 tuns of water, which I can let out upon the overſhot wheel of a mill, and deſcending through a perpendicular of 20 feet, this power applied to proper mechanic inſtruments, will produce a certain effect, that is, it will grind a certain quantity of corn; and that, at a certain rate of expending it, it will grind this corn in an hour. But ſuppoſe the mill equally adapted to produce a proportionable effect, by the application of a greater impulſive power as with a leſs; then, if I let out the water twice as faſt upon the wheel, it will grind the corn twice as faſt, and both the water will be expended and the corn ground in half an hour. Here the ſame mechanical effect is produced; *viz.* the grinding a given quantity of corn, by the ſame mechanical power, *viz.* 1000 tuns of water deſcending through a given perpendicular of 20 feet, and yet this effect is in one caſe produced in half the time of the other. What time, therefore, has to do in the buſineſs is this: let the rate of doing the buſineſs, or producing the effect, be what it will, if this rate is uniform, when I have found by experiment what is done in a given time, then, proceeding at the ſame rate, twice the effect will be produced in twice the time, on ſuppoſition that I have a ſupply of mechanic power to go on with. Thus 1000 tuns of water, deſcending through 20 feet of perpendicular, being, as has been ſhewn, a given mechanic power, let me expend it at what rate I will, if when this is expended, I muſt wait another hour before it be renewed, by the natural flow of a river, or otherwiſe, I can then only expend twelve ſuch quantities of power in 24 hours; but if, while I am expending 1000 tuns in one hour, the ſtream renews me the ſame quantity, then I can expend 24 ſuch quantities of power in 24 hours; that is, I can go on continually at that rate, and the product or effect will be in proportion to time, which is the common meaſure; but the quantity of mechanic power ariſing from the flow of the two rivers, compared by taking an equal portion of time, is double in the one to the other, though each has a mill, that, when going, will grind an equal quantity of corn in an hour.

THE END.

NEW
FUNDAMENTAL EXPERIMENTS
UPON THE
COLLISION OF BODIES.

By Mr. JOHN SMEATON, F. R. S.

EXPERIMENTS

UPON THE

COLLISION OF BODIES.

Read before the Royal Society, April 18, 1782.

IT is univerſally acknowledged, that the firſt ſimple principles of ſcience cannot be too critically examined, in order to their being firmly eſtabliſhed; more eſpecially thoſe which relate to the practical and operative parts of mechanics, upon which much of the active buſineſs of mankind depends. A ſentiment of this kind occaſioned my tract upon *Mechanic Power* which is the ſecond paper of this Volume. What I have now to offer was intended as a ſupplement thereto, and the experiments were then, in part, tried; but the completion thereof was deferred at that time, partly from want of leiſure; partly to avoid too great a length of the paper itſelf; and partly to avoid the bringing forward too many points at once.

My preſent purpoſe is to ſhew, that the true doctrine of the *colliſion of bodies* hangs as it were upon the ſame hook, as the doctrine of the gradual generation of motion from reſt, conſidered in that paper: that is, that whether bodies are put into gradual motion, and uniformly accelerated from reſt to any given velocity; or are put in motion, in an inſtantaneous manner, when bodies of any kind ſtrike one another; the motion, or ſum of the motions produced, has the ſame relation to mechanic power therein defined, which is neceſſary to produce the motion deſired. To prove this, and at the ſame time to ſhew ſome capital miſtakes in principle, which have been *aſſumed* as indiſputable truths by men of great learning, is the reaſon of my now purſuing the ſame ſubject.

I do not mean to point out the particular miſtakes which have been made by particular men, as that would lead me into too great a length: I ſhall therefore content myſelf with obſerving, that the laws of colliſion, which have been inveſtigated by mathematical philoſophers, are principally of three kinds; *viz.* thoſe relating to bodies perfectly *elaſtic*; to bodies perfectly unelaſtic, and perfectly *ſoft*; and to bodies perfectly unelaſtic, and perfectly *hard.* To avoid prolixity, I ſhall conſider in each, only the ſimple caſe of two bodies which are equal in weight or quantity of matter ſtriking one another. Reſpecting thoſe which are perfectly elaſtic, it is univerſally agreed, that when two ſuch bodies ſtrike one another, no motion is loſt; but that in all caſes, what is loſt by one is acquired by the other: and hence, that if an elaſtic body in motion ſtrikes another at reſt, upon the ſtroke the former will be reduced to a ſtate of reſt, and the latter will fly off with an equal velocity.

In like manner, if a non-elaſtic *ſoft* body ſtrikes another at reſt, they neither of them remain at reſt, but proceed together from the point of colliſion with exactly one half of the velocity that the firſt had before the ſtroke; this is alſo univerſally allowed to be true, and is fully proved by every good experiment upon the ſubject.

Reſpecting the third ſpecies of body, that is, thoſe that are non-claſtic and yet perfectly hard: the laws of motion relating to them, as laid down by one ſpecies of philoſophers, have been rejected by another; the latter alledging, that there are no ſuch bodies to be found in nature whereon to try the experiment; but thoſe who have laid down and aſſigned the doctrine that would attend the colliſion of bodies, of this kind (if they could be found) have univerſally agreed, that if a non-elaſtic *hard* body was to ſtrike another of the ſame kind at reſt, that, in the ſame manner as is agreed concerning non-elaſtic ſoft bodies, they neither of them would remain at reſt, but would in like manner proceed from the point of colliſion, with exactly one half of the velocity that the firſt had before the ſtroke: in ſhort, they lay it down as a rule attending all non-elaſtic bodies, whether hard or ſoft, that the velocity after the ſtroke will be the ſame in both, *viz. one half* of the velocity of the original ſtriking body.

Here is therefore the aſſumption of a principle, which in reality is proved by no experiment, nor by any fair deduction of reaſon that I know of, *viz.* that the velocity of non elaſtic *hard* bodies after the ſtroke muſt be the ſame as that reſulting from the ſtroke of non-elaſtic *ſoft* bodies; and the queſtion now is, whether it is true or not?

Here it may be very properly aſked, what ill effects can reſult to practical men, if philoſophers ſhould reaſon wrong concerning the effects of what does not exiſt in nature, ſince the practical men can have no ſuch materials to work upon, or miſjudge of? But it is anſwered, that they who infer an equality of effects between the two ſorts, may from thence be miſled themſelves, and in conſequence miſlead practical men in their reaſonings and concluſions concerning the ſort with which they have abundant concern, to wit, the non-elaſtic *ſoft bodies*, of which water is one, which they have much to do with in their daily practice.

Previous to the trying my experiment on mills, I never had doubted the truth of the doctrine, that the ſame velocity reſulted from the ſtroke of both ſorts of non-elaſtic bodies; but the trial of thoſe experiments made me clearly ſee at leaſt the inconcluſiveneſs, if not the falſity of that doctrine: becauſe I found a reſult which I did not expect to have ariſen from either ſort; and for the which, when it appeared from experiment, I could ſee a ſubſtantial reaſon why it ſhould take place in one ſort, and that it was impoſſible that it could take place in the other; for if it did, the bodies could not have been perfectly *hard*, which would be contrary to the hypotheſis: of this deduction I have given notice in my ſaid tract on mills, page 30. *The effect therefore of overſhot wheels, &c.*

It may alſo be ſaid, that ſince we have no bodies perfectly elaſtic, or perfectly unelaſtic and *ſoft*, why ſhould we expect bodies perfectly unelaſtic and *hard?* Why may not the effects be ſuch as ſhould reſult from a ſuppoſition of their being *imperfectly elaſtic* joined with their being *imperfectly hard?* But here I muſt obſerve, that the ſuppoſition appears to be a contradiction in terms.

We have bodies which are ſo nearly perfectly elaſtic, that the laws may be very well deduced and confirmed by them; and the ſame obtains with reſpect to non-elaſtic *ſoft* bodies; but concerning bodies of a mixed nature, which are by far the greateſt number, ſo far as they are wanting in elaſticity, they *are ſoft* and *bruiſe*, *yield*, or *leave a mark* in colliſion; and ſo far as they are not perfectly ſoft they are elaſtic, and obſerve a mixture of the law relative to each; but imperfectly elaſtic bodies, imperfectly hard come in reality under the *ſame deſcription* as the former mixed bodies: for ſo far as they are imperfectly hard they are ſoft, and either *bruiſe* and *yield*, or leave a mark in the ſtroke; and ſo far as they want perfect elaſticity, they are non-elaſtic; that is to ſay, they are bodies imperfectly elaſtic, and imperfectly ſoft; and in fact I have never yet ſeen any bodies but what come under this deſcription. It ſeems, therefore, that reſpecting the *hardneſs* of bodies they differ in degrees of it, in proportion as they have a greater degree of tenacity or coheſion; that is, are further removed from perfect ſoftneſs, at the ſame time that their elaſtic ſprings, ſo far as they reach, are very ſtiff; and hence we may (by the way) conclude, that the ſame mechanic power that is required to change the figure in a *ſmall degree* of thoſe bodies that have the popular appellation of *hard bodies*, would change it in a *great degree* in thoſe bodies that approach towards ſoftneſs, by having a ſmall degree of tenacity or coheſion. In the former kind we may rank the harder kinds of *caſt iron*, and in the latter, *ſoft tempered clay*.

While the philoſophical world was divided by the diſpute about the *old* and *new opinion*, as it was called, concerning the powers of bodies in motion, in proportion to their different velocities: thoſe who held the old opinion contending, that it was as the velocity *ſimply*, aſked thoſe of the new, How, upon their principles, they would get rid of the concluſions ariſing from the doctrine of unelaſtic perfectly hard bodies? They replied, they found no ſuch bodies in nature, and therefore did not concern themſelves about them. On the other hand, thoſe of the new opinion aſked thoſe of the old, How they would account for the caſe of non-elaſtic ſoft bodies, where, according to them, the whole motion loſt by the ſtriking body was retained in the two after the ſtroke (the two bodies moving together with the half velocity,) though the two non-elaſtic bodies had been bruiſed and changed their figure by the ſtroke; for, if no motion was loſt, the change of figure muſt be an effect without a cauſe? To obviate this, thoſe of the old opinion ſeriouſly ſet about proving, that the bodies might change their figure, without any loſs of motion in either of the ſtriking bodies.

Neither of theſe anſwers have appeared to me ſatisfactory, eſpecially ſince my mill experiments: for with reſpect to the firſt, it is no proper argument to urge the impoſſibility of finding the proper material for an experiment, in anſwer to a concluſion drawn from an abſtract idea. On the other hand, if it can be ſhewn, that the figure of a body can be changed, without a *power*, then, by the ſame law, we might be able to make a *forge hammer* work upon a maſs of ſoft iron, without any other power than that neceſſary to overcome the friction, reſiſtance, and original *vis inertia*, of the parts of the machine to be put in motion: for, as no progreſſive motion is given the maſs of iron by the hammer (it being ſupported by the anvil), no power can be expended that way; and if none is loſt to the hammer from changing the figure of the iron, which is the only effect produced, then the whole power muſt reſide in the hammer, and it would jump back again to the place from which it fell, juſt in the ſame manner as if it fell upon a body perfectly elaſtic, upon which, if it did fall, the caſe would really happen: the power therefore, to work the hammer would be the ſame, whether it fell upon an elaſtic or non-elaſtic body; an idea ſo very contrary to all experience, and even apprehenſion, of both the philoſopher and vulgar artiſt, that I ſhall here leave it to its own condemnation.

As nothing, however, is ſo convincing to the mind as experiments obvious to the ſenſes, I was very deſirous of contriving an experiment in point, and as I ſaw no hopes of finding matter to make a *direct* experiment, I turned my mind towards an indirect one: ſo circumſcribed, however, as to prove inconteſtably, that the reſult of the ſtroke of two non-elaſtic perfectly hard bodies could not be the ſame as would reſult from the colliſion of two ſoft ones; that is, if it can be *bona fide* proved, that *one half* of the original power is loſt in the ſtroke of ſoft bodies by the change of figure (as was very ſtrongly ſuggeſted by the mill experiments); then ſince no ſuch loſs can happen in the colliſion of bodies perfectly hard, the reſult and conſequence of ſuch a ſtroke muſt be *different*.

The conſequence of a ſtroke of bodies perfectly hard, but void of elaſticity, muſt doubtleſs be different from that of bodies perfectly *elaſtic:* for having no ſpring the body at reſt could not be driven off with the velocity of the ſtriking body, for that is the conſequence of the action of the ſpring or elaſtic parts between them, as will be ſhewn in the reſult of the experiments; the ſtriking body will therefore not be ſtopped, and as the motion it loſes muſt be communicated to the other, from the equality of action and re-action, they will both proceed together, with an equal velocity, as in the caſe of non-elaſtic ſoft bodies: the queſtion, therefore, that remains is, what that *velocity muſt be?* It muſt be greater than that of the non-elaſtic ſoft bodies, becauſe there is no mechanical power loſt in the ſtroke. It muſt be leſs than that of the ſtriking body, becauſe, if equal, inſtead of a *loſs* of motion by the colliſion it will be doubled. If, therefore, non-elaſtic ſoft bodies loſe half their motion, or mechanical power, by change of figure in colliſion, and yet proceed together with half the velocity, and the non-elaſtic hard bodies can loſe *none* in any manner whatever; then, as they muſt move together, their velocity muſt be ſuch as to preſerve the equality of the mechanic power, *unimpaired*, after the ſtroke the ſame as it was before it.

For example, let the velocity of the ſtriking body before the ſtroke be 20, and its maſs or quantity of matter 8; then, according to the rule deduced from the experiments in the tract on *Mechanic Power* (ſee exp. third and fourth) that power will be expreſſed by 20×20=400, which × 8=3200; and if half of it is loſt in the ſtroke, in the caſe of non-elaſtic ſoft bodies, it will be reduced to 1600; which ÷ 16 the double quantity of matter, will give 100 for the ſquare of their velocity; the ſquare root of which being 10, will be the velocity of the two non-elaſtic ſoft bodies after the ſtroke, being juſt one half of the original velocity, as it is conſtantly found to be. But in the non-elaſtic hard bodies, no power being loſt in the ſtroke, the mechanic power will remain after it, as before it, = 3200; this, in like manner, being divided by 16, the double quantity of matter, will give 200 for the ſquare of the velocity, the ſquare root of which is 14.14, &c. for their velocity after the ſtroke, which is to 10, the velocity of the non-elaſtic ſoft bodies after the ſtroke, as the ſquare root of 2 to 1, or as the diagonal of a ſquare to its ſide.

It remains, therefore, now to be proved, that preciſely half of the mechanic power is *loſt* in the colliſion of non-elaſtic ſoft bodies; for which purpoſe my mind ſuggeſted the following reflections. In the colliſion of elaſtic bodies the effect ſeemingly inſtantaneous, is yet performed in *time*; during which time the natural ſprings reſiding in elaſtic bodies, and which conſtitute them ſuch, are bent or forced, till the motion of the ſtriking body is divided between itſelf and the body at reſt; and in this ſtate the two bodies would then proceed together, as in the caſe of non-elaſtic ſoft bodies; but as the ſprings will immediately reſtore themſelves in an equal time, and with the ſame degree of *impulſive force*, wherewith they were bent in this re-action, the motion that remained in the ſtriking body will be totally deſtroyed, and the total exertion of the two ſprings, communicated to the original reſting body, will cauſe it to fly off with the ſame velocity wherewith it was ſtruck.

Upon this idea, if we could conſtruct a couple of bodies in ſuch a way that they ſhould either act as bodies perfectly elaſtic; or that their ſprings ſhould at pleaſure be hooked up, retained, or prevented from reſtoring themſelves, when at their extreme degree of bending; and if the bodies under theſe circumſtances obſerved the laws of colliſion of non-elaſtic ſoft bodies, then it would be proved, that one half of the mechanical power, reſiding in the ſtriking body, would be loſt in the action of colliſion; becauſe the impulſive force or power of the ſpring in its reſtitution being cut off, or ſuſpended from acting, which is equal to the impulſive force or power to bend it (and which alone has been employed to communicate motion from one body to the other), it would make it evident, that one half of the impulſive force is loſt in the action, as the other half remains *locked up* in the ſprings, it alſo follows, as a *collateral circumſtance*, that be the impulſive power of the ſprings what it may from firſt to laſt, yet as one half of the *time* of the action is by this means cut off, in this ſenſe alſo it will follow, that one half of the mechanic power is deſtroyed; or rather, in this caſe, remains locked up in the ſprings, capable of being *re-exerted* whenever they are ſet at liberty, and of producing a freſh mechanical effect, equivalent to the motion or mechanical power of the two non-elaſtic ſoft bodies after their colliſion.

Hence we muſt infer, that the quantity of mechanical power expended in diſplacing the parts of non-elaſtic ſoft bodies in colliſion, is exactly the ſame as that expended in bending the ſprings of perfectly elaſtic bodies; but the difference in the ultimate effect is, that in the non-elaſtic ſoft bodies, the power taken to diſplace the parts will be totally loſt and deſtroyed, as it would require an equal mechanic power to be raiſed a-freſh, and exerted in a contrary direction to reſtore the parts back again to their former places; whereas, in the caſe of the elaſtic bodies, the operation of half the mechanic power is, as obſerved already, only locked up and ſuſpended, and capable of being re-exerted without a further original acceſſion.

Theſe ideas aroſe from the reſult of the experiments tried upon the machine deſcribed in my ſaid tract upon mechanic power, and were alſo communicated to my very worthy and ingenious friend WILLIAM RUSSEL, Eſq. F. R. S. at the ſame time that I ſhewed him thoſe experiments in 1759; but the mode of putting this matter to a full and fair mechanical trial has ſince occurred; and though ſome rough trials, ſufficient to ſhew the effect, were made thereon, prior to the offering the Paper on Mechanical Power to the Society in 1776, yet the machine itſelf I had not leiſure to compleat to my ſatisfaction till lately; which I mention to apologize for the length of time that theſe ſpeculations have taken in bringing forward.

DESCRIPTION OF THE MACHINE FOR COLLISION.

FIG. I. ſhews the front of the Machine as it appears at reſt when fitted for uſe.

A is the pedeſtal, and A B the pillar, which ſupports the whole, C D are two compound bodies of about a pound weight each, but as nearly equal in weight as may be. Theſe bodies are alike in conſtruction, which will be more particularly explained by fig. 2. Theſe bodies are ſuſpended by two white fir rods of about half an inch diameter, *e f* and *g h* being about four feet long from the point of ſuſpenſion to the center of the bodies; and their ſuſpenſion is upon the croſs piece I I, which is mortiſed through, to let the rods paſs with perfect freedom; and they hang upon two ſmall plates filed to an edge on the under ſide, and paſs through the upper part of the rods. Their centers are at *k* and *l*, and the edges being let into a little notch, on each ſide the mortoiſe, the rods are at liberty to vibrate freely upon their reſpective points (or rather edges) of ſuſpenſion, and are determined to one plain of vibration. M N is a flat arch of white wood, which may be covered with paper, that the marks thereupon may be more conſpicuous.

The croſs piece I I is made to project ſo far before the pillar, that the bodies in their vibrations may paſs clear of it, without danger of ſtriking it; and alſo the arch M N is brought ſo far forward as to leave no more than a clearance, ſufficient for the rods to vibrate freely without touching it.

FIG. 2. ſhews one of the compound bodies, drawn of its full ſize. A B is a block of wood, and about as much in breadth as it is repreſented in height, through a hole in which the wood rod C C paſſes, and is fixed therein.

D B repreſents a plate of lead about three-eighths of an inch thick, one on each ſide, ſcrewed on by way of giving it a competent weight. *d* B *e f g* repreſents the edge of a ſpringing plate of braſs, rendered elaſtic by hard hammering; it is about five eighths of an inch in breadth, and about one twentieth of an inch thick. It is fixed down upon the wooden block at its end *d* B by means of a bridge plate, whoſe end is ſhewn *h i*, and is ſcrewed down on each ſide of the ſpring plate by ſcrews, which being relaxed the ſpring can be taken out at pleaſure, and adjuſted to its proper ſituation. *k l* is a light thin ſlip of a plate, whoſe under edge is cut into teeth like a fine ſaw or ratchet, and is attached to the ſpring by a pin at *k*, which paſſes through it, and alſo through a ſmall ſtud rivetted into the back part of the ſpring, and upon which pin, as a center, it is freely moveable. *m n* ſhews a ſmall plate or ſtud ſeen edgeways raiſed upon the bridge plate, through an hole in which ſtud the ratchet paſſes; and the lower part of the hole is cut to a tooth ſhaped properly to catch the teeth of the ratchet, and retain it together with the ſpring at any degree to which it may be ſuddenly bent; and for this intent it is kept bearing gently downward, by means of a wire-ſpring *o p q*, which is in reality double, the bearing part at *o* being ſemi-circular; from which branching off on each ſide the rod C C, paſſes to *p*, and fixes at each end into the wood at *q*. However to clear the ratchet, which is neceſſarily in the middle as well as the rod, the latter is perforated; and alſo the block is cut away, ſo far as to ſet the mainſpring at *e* free of all obſtacles that would prevent its play from the point B. The part *f g* is ſhewn thicker than the reſt, by being covered with thin kid leather tight ſewed on, to prevent a certain jarring that otherwiſe takes place on the meeting of the ſprings in colliſion.

Let us now return to *fig.* 1. the marks upon the arch M N are put on as follows. *o p* is an arch of a circle from the center *l*, and *q r* an arch of a circle from the center *k*, interſecting each other at S. Now the middle line of the marks *t*, *v*, are at the ſame diſtance from the middle line at S that the centers *k l* are; ſo that when each body hangs in its own free poſition, without bearing againſt the other, the rod ef will cover the mark at t, and the rod gh will cover the mark at v. From the point S upon the arches Sp and Sq reſpectively, ſet off points at an equal and competent diſtance from S each way, which will give the middle of the mark w and x: and upon the arch Sp find the middle point between the mark v and w, which let be y; and on the other ſide, in like manner, upon the arch Sq find a middle point for the mark z; then ſet off the diſtance Sv or St from y each way, and from z each way; and from theſe points, drawing lines to tbe reſpective centers l and k, they will give the place and poſition of the marks a, b, and c, d; and thus is the machine prepared for uſe.

FOR TRIALS ON ELASTIC BODIES.

FOR this uſe take out the pins and ratchets from each reſpectively, and the ſprings being then at liberty, with a ſhort bit of ſtick (ſuppoſe the ſame ſize as the rods) turn aſide the rod gh with the right hand, carrying the body D upwards till the ſtick is upon the mark w, as ſuppoſe at ⊖; there hold it, and with the left ſet the body C perfectly at reſt; in which caſe the rod ef will be over the mark t; then ſuddenly withdraw the ſtick, in the direction that the rod gh is to follow it, and the ſpring of the body D, impinging upon that of the body C, they will be both bent, and alſo reſtored; and the body C will fly off, and mount till its rod ef covers the mark x; the rod of the ſtriking body D remaining at reſt upon its proper mark of reſt v, till the body C returns, when the body D will fly off in the ſame manner; the two bodies thus rebounding a number of times, loſing a part of their vibration each time; but ſo nearly is the theory of elaſtic bodies fulfilled hereby, that the ſingle advantage of originally puſhing the rod gh beyond the mark w, by the thickneſs of the ſtick, or its own thickneſs, is ſufficient to carry the rod of the quieſcent body C completely to its mark x.

There are ſeveral other experiments which may be made with this apparatus, in confirmation of the doctrine of the colliſion of elaſtic bodies; which being univerſally agreed upon, and well known, it is needleſs further to dwell upon here; but reſpecting the application to non-elaſtic ſoft bodies, it is far more difficult to come at a fitneſs of materials for this kind of experiment, than it is for thoſe ſuppoſing perfectly elaſticity. The concluſions, however, may be attained with equal certainty.

FOR TRIALS ON NON-ELASTIC SOFT BODIES:

FOR this purpoſe the ratchets muſt be applied and put in order as before deſcribed, and the ſprings being both put to their point of reſt, let the body D be put to its mark *w* in the ſame manner as before deſcribed, and the body C to reſt. The body D being let go, and ſtriking the body C at reſt, in conſequence of the ſtroke, the ſprings being hooked up by the ratchets, they both move from their reſting marks *t*, *v*, reſpectively towards M: Now if they both moved together, and the rod *e f* covered the mark *c*, and the rod *g h* covered the mark *d* at their utmoſt limit, then they would truly obey the laws of non-elaſtic ſoft bodies; becauſe their medium aſcent would be to the mark *z*, which is juſt half the angle of aſcent to the mark *x*; but as in this piece of machinery, though the main or principal ſprings are hooked up, yet every part of them, and all the materials of which they are compoſed, and to which they are attached, have a degree, or more properly ſpeaking, a certain compaſs of *elaſticity*, which, as ſuch, is perfect, and no motion loſt thereby.

We muſt not, therefore, expect the two compound bodies after the ſtroke to ſtick together without ſeparating, as would be the caſe with bodies truly non-elaſtic and ſoft; but that from the elaſticity they are poſſeſſed of, they will by rebounding be ſeparated; but that elaſticity being perfect, can occaſion no loſs of motion to the ſum of the two bodies; ſo that if the body C aſcends as much above its mark *c* as the body D falls ſhort of its mark d, then it will follow, that their medium aſcent will ſtill be to the mark z, as it ought to have been, had they been truly non-elaſtic ſoft bodies; and this, in reality, is truly the caſe in the experiment, as nearly as it can be diſcerned.

After a few vibrations, by the rubbing of the ſprings againſt one another, they are ſoon brought to reſt; and here they would *always reſt* had they been truly and properly perfect non-elaſtic ſoft bodies; but here, as in the caſe of theſe bodies, by a change of the figure and ſituation of the component parts, there is expended one half of the mechanical power of the firſt mover, yet in this caſe the other half is not *loſt*, but *ſuſpended* ready to be reexerted whenever it is ſet at liberty; and that it is really and *bona fida one half*, and neither more or leſs appears from this uncontroverted ſimple principle, that the power of reſtitution of a perfect ſpring is exactly equal to the power that bends it. And this may, in a certain degree, be ſhewn to be fact by experiment, if there were any need of ſuch a proof; for if, when the bodies are at reſt after the laſt experiment the two rods are laſhed together the bottom with a bit of thread, and then the ratchets unpinned and removed; on cutting the thread with a pair of ſciſſars they will each of them rebound, C towards M and D towards N, and if they rebounded reſpectively to z and y, the mechanical power exerted would be the ſame as it was after the ſtroke, when the mean of their two aſcents was up to the mark Z; but here it is not to be expected, becauſe not only the motion loſt by the friction of the ratches is to be deducted, becauſe it had the effect of real non-elaſticity; but alſo the elaſticity that ſeparated them in the ſtroke, which was loſt in the vibrations that ſucceeded; neither of which hindered the mean aſcent to be to z; but yet, under all theſe diſadvantages in the machine (if not unreaſonably ill made) the rod $e f$ will aſcend to d, and $g h$ to a: and hence I infer, as a poſitive truth, that in the colliſion of non-elaſtic ſoft bodies, *one half of the mechanic power reſiding in the ſtriking body is loſt in the ſtroke.*

Reſpecting bodies unelaſtic and perfectly hard, we muſt infer, that ſince we are unavoidably led to a concluſion concerning them, which contradicts what is eſteemed a truth capable of the ſtricteſt demonſtration; viz. that the velocity of the center of gravity of no ſyſtem of bodies can be changed by any colliſion betwixt one another, ſomething muſt be aſſumed that involves a contradiction. This perfectly holds, according to all the eſtabliſhed rules, both of perfectly elaſtic and perfectly non-elaſtic *ſoft* bodies; rules which muſt fail in the perfectly non-elaſtic *hard* bodies, if their velocity after the ſtroke is to the velocity of the ſtriking body as one is to the ſquare root of 2; for then the center of gravity of the two bodies will by the ſtroke acquire a velocity greater than the center of gravity the two bodies had before the ſtroke in that proportion, which is proved thus.

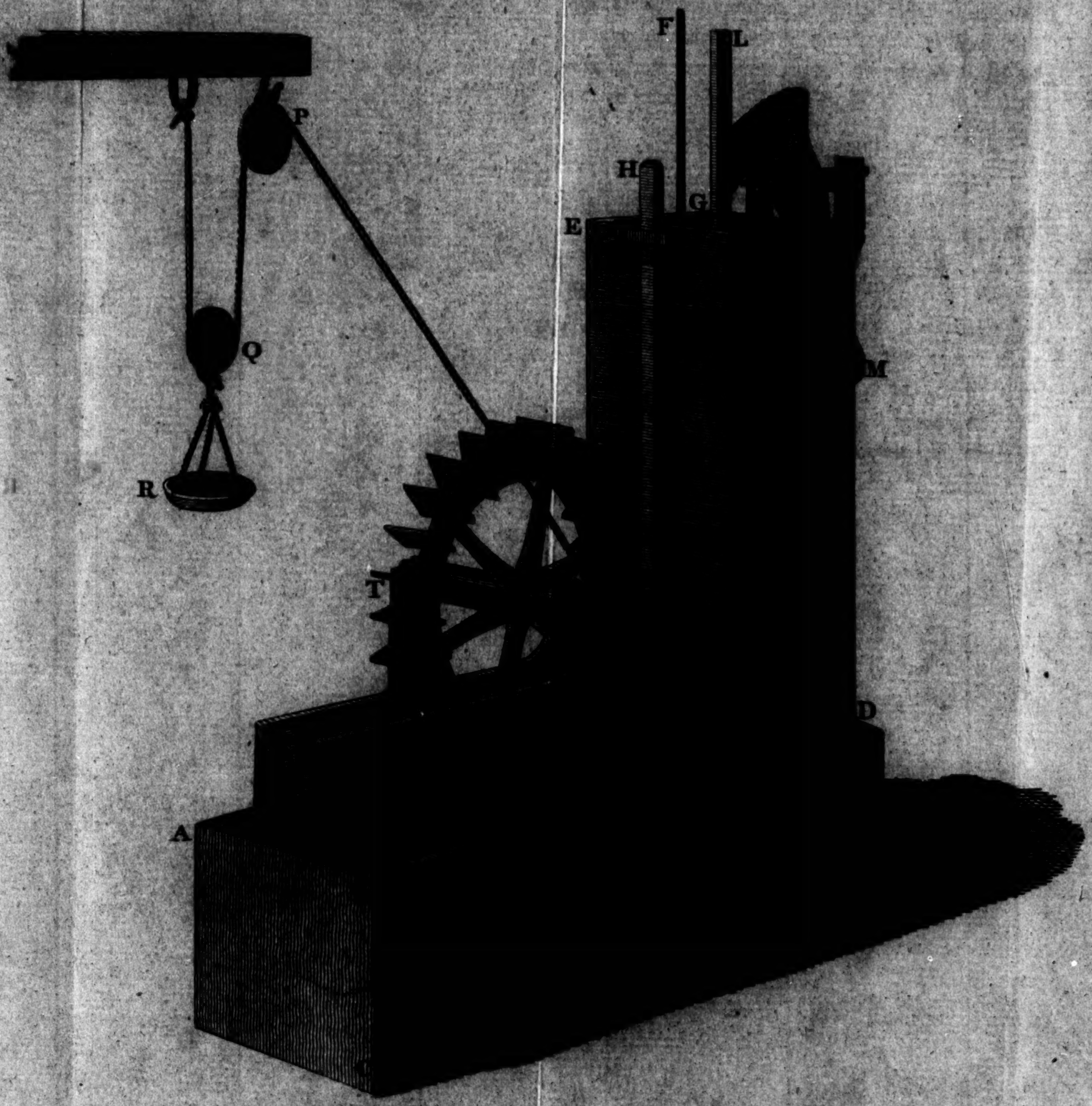
At the outſet of the ſtriking body, the center of gravity of the two bodies in our caſe will be exactly in the middle between the two; and when they meet it will have moved from their half diſtance to their point of contact, ſo the velocity of the center of gravity before the bodies meet will be exactly one half of the velocity of the ſtriking body; and, therefore, if the velocity of the ſtriking body is 2, the velocity of the center of gravity of both will be one. After the ſtroke, as both bodies are ſuppoſed to move in contact, the velocity of the center of gravity will be the ſame as that of the bodies; and as their velocity is proved to be the ſquare root of 2, the velocity of their center of gravity will be increaſed from 1. to the ſquare root of 2; that is, from 1. to 1.414, &c.

The fair inference from theſe contradictory concluſions therefore is, that an unelaſtic hard body (perfectly ſo) is a repugnant idea, and contains in itſelf a contradiction; for to make it agree with the fair concluſions that may be drawn on each ſide, from clear premiſes, we ſhall be obliged to define its properties thus: That in the ſtroke of unelaſtic hard bodies they can not *poſſibly* *loſe any* mechanic power in the ſtroke; becauſe no other impreſſion is made than the communication of motion; and yet they *muſt loſe* a *quantity* of mechanic power in the ſtroke; becauſe, if they do not, their common center of gravity, as above ſhewn, will acquire an *increaſe* of velocity by their ſtroke upon each other.

In a like manner the idea of a *perpetual motion* perhaps, at firſt ſight, may not appear to involve a contradiction in terms; but we ſhall be obliged to confeſs that it does, when, on examining its requiſites for execution, we find we ſhall want bodies having the following properties; that when they are made to *aſcend* againſt gravitation their abſolute weight ſhall be *leſs*; and when they *deſcend* by gravitation (through an equal ſpace) their abſolute weight ſhall be greater; which, according to all we know of nature, is a *repugnant* or *contradictory idea.*

FINIS.

Smeaton's Experiments. Plate I.



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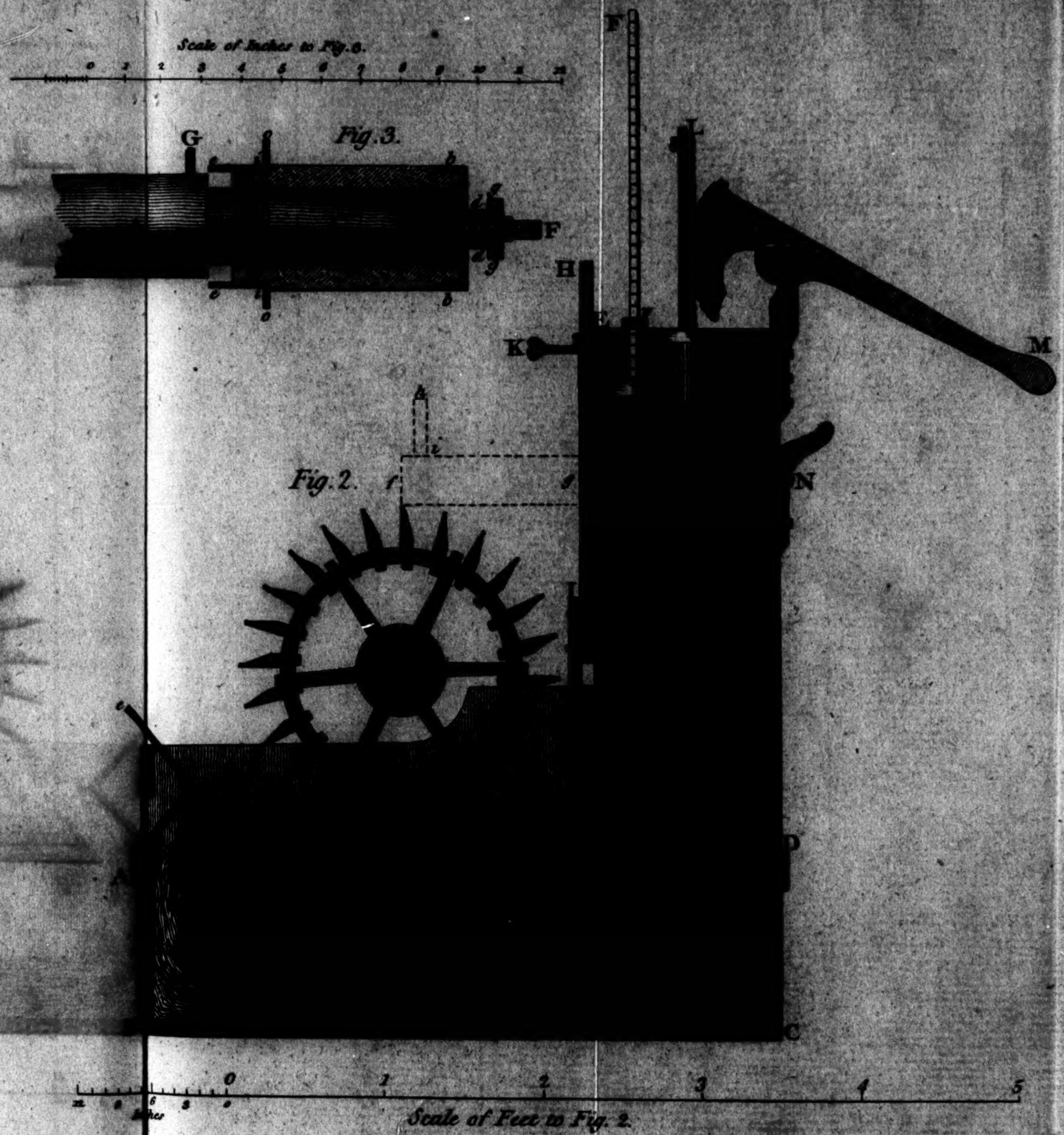

Fig. 3.

Fig. 2.

Scale of Feet to Fig. 2.

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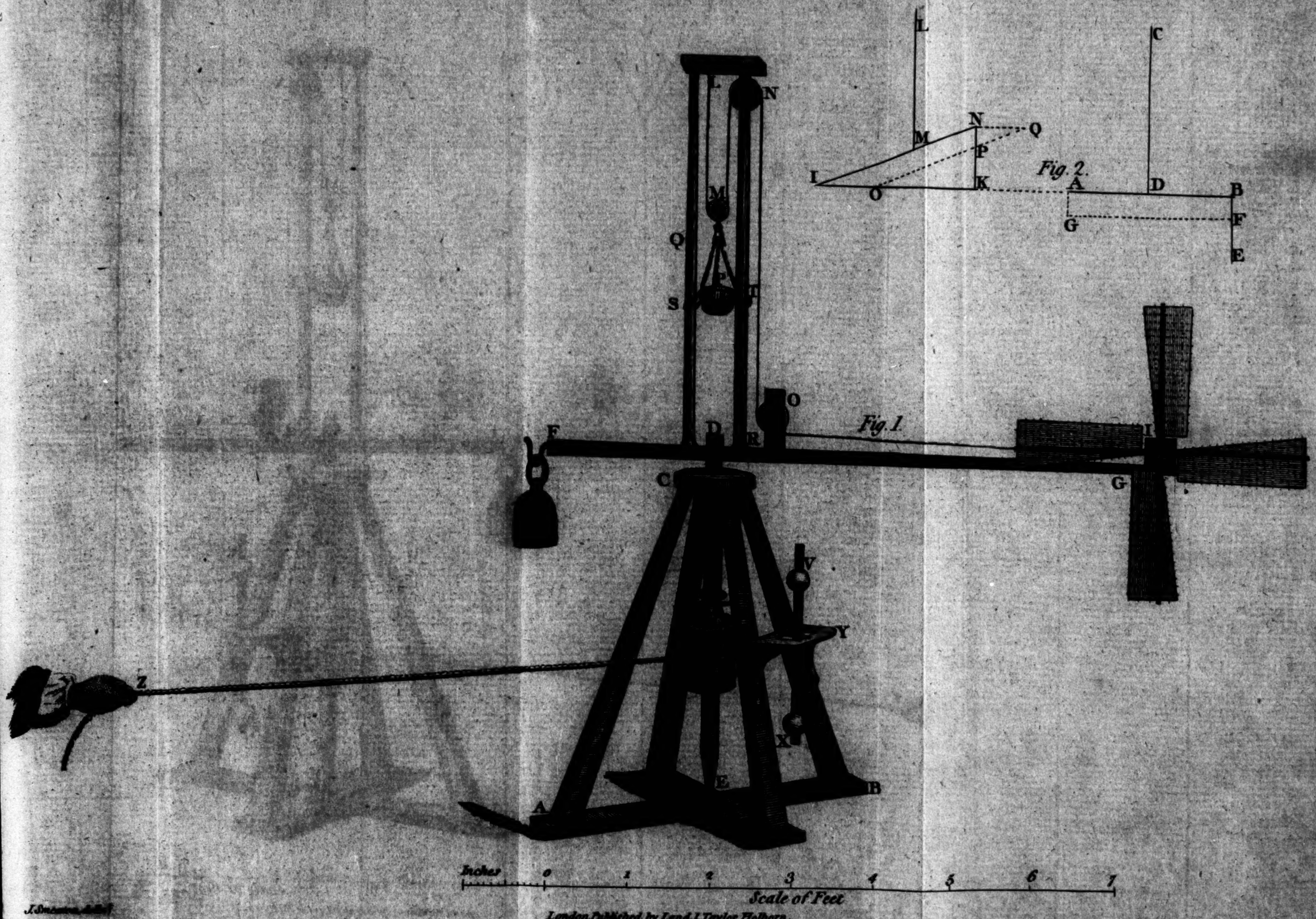

Fig. 2.

Fig. 1.

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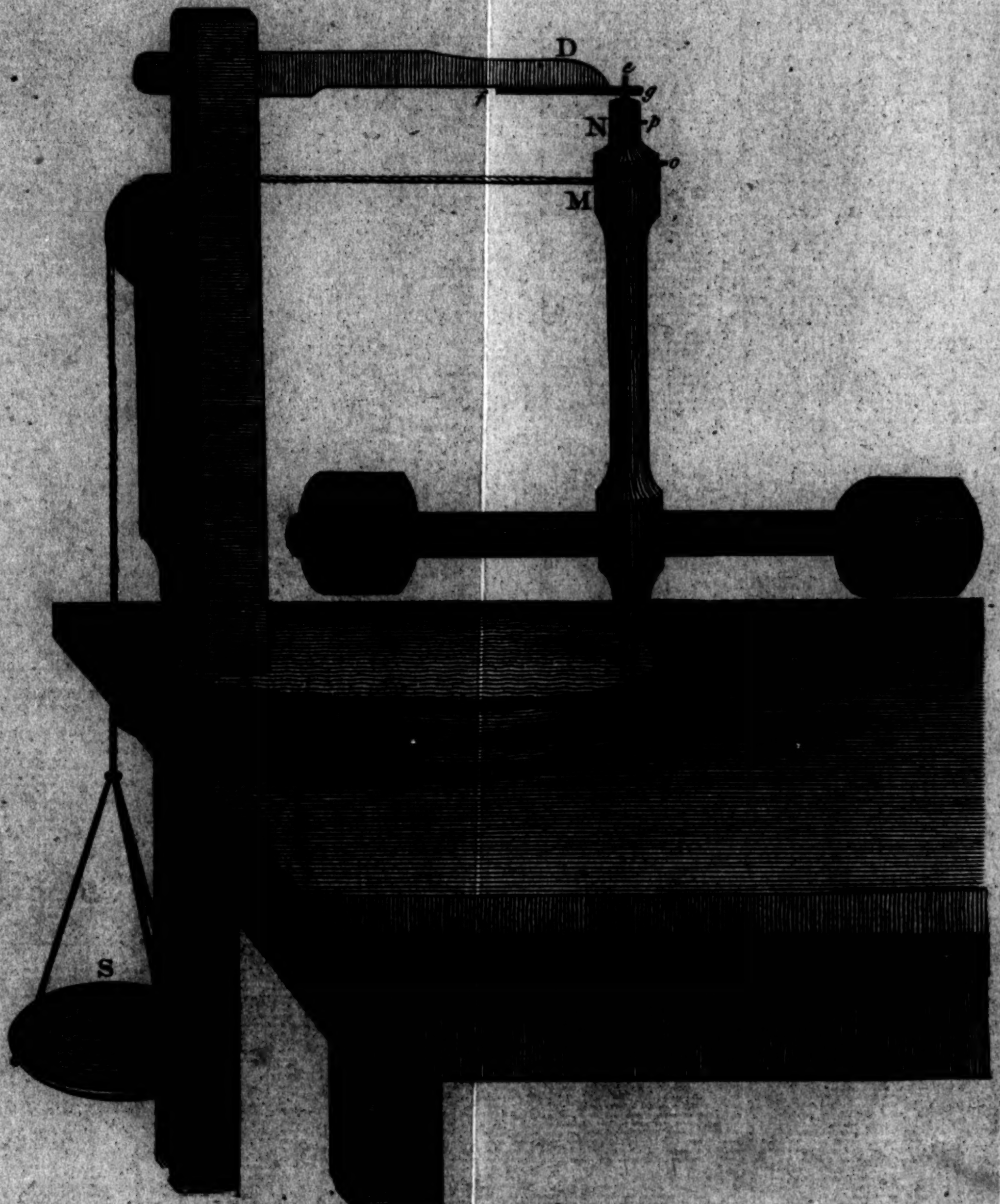
Smeaton's Experiments, Plate IV.



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MACHINE for COLLISION.

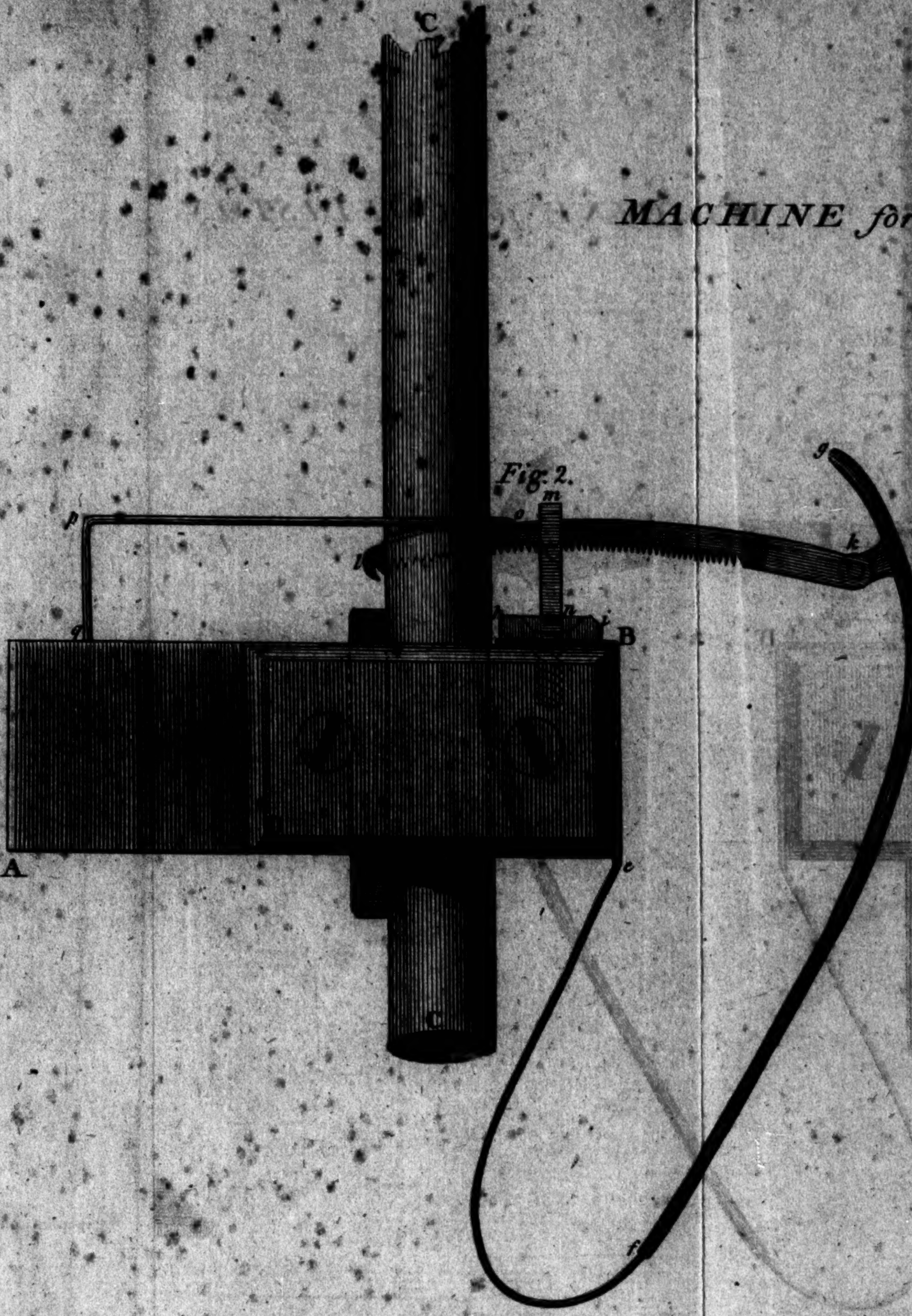
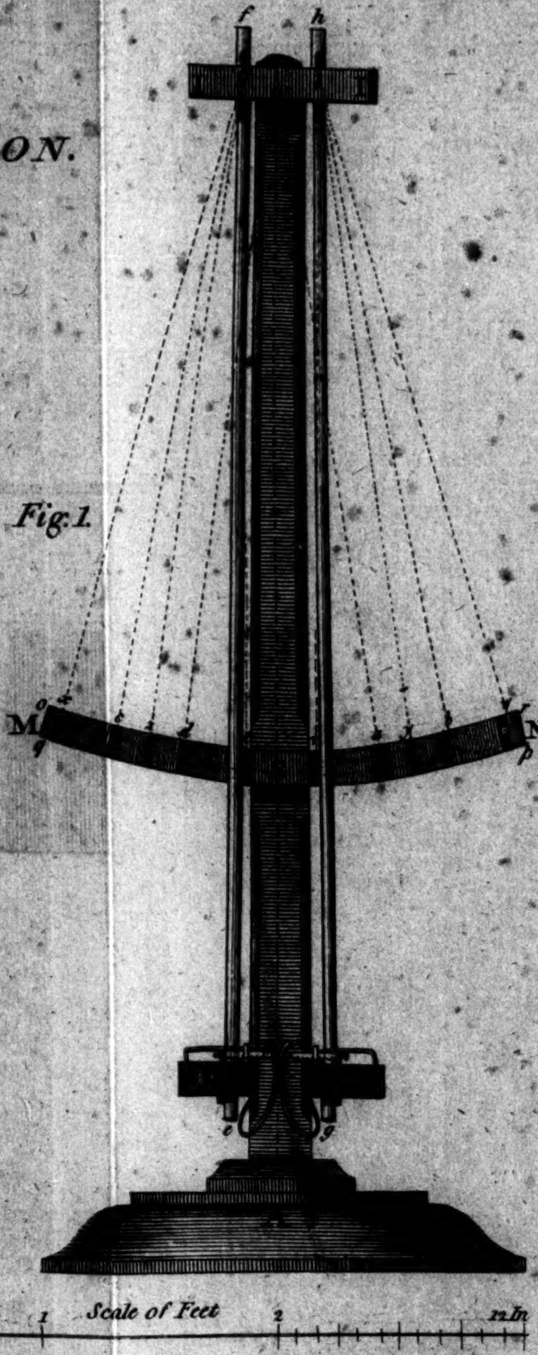


Fig. 2.

Fig. 1.



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